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ABSTRACT

Ten delinquency prevention studies are reviewed that incorporated rigorous evaluative procedures (specifically the classic experimental design) for assessing programmatic outcomes. Following an introduction, the evaluation mechanisms built into each project are described, since they were used for determination of the effectiveness of the preventive services. The format used for the project reviews is then outlined: background (how and why the experiment was undertaken), theoretical orientations of service given, research design, treatment providers, treatment population, dimensions of treatment (amount of contact time, treatment plan, involvement of experimental subjects), findings, and recommendations. The ten studies reviewed are Cambridge-Somerville Youth Study, Massachusetts; New York City Youth Board, Validation Study of the Glueck Prediction Table; Minimum Benefits Project, Washington, D.C.; Midcity Project: Boston, Massachusetts; Youth Consultation Service Project, New York, New York; Chicago Youth Development Project, Illinois; Seattle Atlantic Street Center Experiment, Washington; Youth Development Program, Columbus, Ohio; Opportunities for Youth Project, Seattle, Washington; Wincroft Youth Project, Manchester, England. A general discussion uses the same outline/format topics to consider the experiments as a whole. The generalized finding is made that no study produced positive effects, that is, the delinquency prevention services were no more effective than an absence of services. (YLB)

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Reports of the National Juvenile Justice Assessment Centers

Juvenile Delinquency Prevention Experiments:

A Review and Analysis

by
William C. Berleman

October 1980

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A review of the ten delinquency prevention studies which utilized the classic experimental design (marked experimental and control groups) reveals no study produced positive results; the listed delinquency prevention services were no more effective than an absence of services. While this generalized finding is well-known, a concise yet comprehensive analysis of each delinquency prevention experiment has not been heretofore available, so that the similarities and differences of the experiments were not understood.

A common outline is used in presenting each experiment: 1) background (how and why the experiment was undertaken); 2) theoretical orientations of the service given; 3) the research design; 4) who the treatment providers were; 5) characteristics of the treatment population; 6) specific dimensions of the service given, namely a) amount of contact time, b) the treatment plan, c) the involvement of the experimental subjects; 7) the findings; and 8) the recommendations (when made) of the study staff.

A final section, following the outline used to illuminate each experiment, draws all experiments together in order to discuss the differences, similarities, shortcomings, strengths, and persisting legal and procedural dilemmas which characterize the experiments taken together. This discussion provides some interesting findings, such as that contact with experimental subjects by treatment providers is in most experiments extremely modest (less than three contacts per month); and that a preponderance of all experimental subjects has been non-white.

This review is meant for practitioners and planners who wish a quick but not superficial understanding of the treatment and evaluative procedures used in past delinquency prevention experiments.

I. INTRODUCTION

A prudent assessment of what is known about the effectiveness of efforts aimed at averting delinquent or antisocial behavior might well begin with a review of those delinquency prevention programs that incorporated rigorous evaluative procedures for assessing programmatic outcomes. The aim here is to provide such a review and to set forth major questions raised by it.

Because most programs claiming to prevent delinquency or antisocial behavior are not subject to evaluative research, the number of projects reviewed is relatively small. Ten projects have been identified (see Table 1). These projects, it should be understood, do not constitute any final or even comprehensive statement about what may conceivably be done to prevent delinquency. As will be seen, these prevention experiments tend to focus upon youthful subjects identified as needing preventive treatment regimen; the aim usually was to change the antisocial behavior of the individual. This kind of circumscribed effort lends itself to evaluative research. The programs reviewed, then, do not reflect the range of speculative richness the term "delinquency prevention" nourishes nor the many programs which have been launched in the name of delinquency prevention.

For our purposes, the delinquency prevention programs to be considered will 1) have provided a service for children

TABLE 1

DELINQUENCY PREVENTION EXPERIMENTS, 1937-1968

	Title	Place	Years	Subjects		Overall Evaluation of Service
				Experimental	Control	
1.	Cambridge-Somerville Youth Study	Cambridge-Somerville Massachusetts	1937-45	325	325	Ineffective
2.	New York City Youth Board Validation of Prediction Scale	New York, N.Y.	1952-57	29	29	Ineffective
3.	Maximum Benefits Project	Washington, D.C.	1954-57	111	68	Ineffective
4.	Midcity Project	Boston, Masschusetts	1954-57	205	Ca. 112 (Estimate)	Ineffective
5.	Youth Consolation Service	New York, N.Y.	1955-60	189	192	Ineffective
6.	Chicago Youth Development Project	Chicago, Ill.	1961-66	Unknown		Ineffective
7.	Seattle Atlantic Street Center Experiment	Seattle, Wash.	1962-68	52	50	Ineffective
8.	Youth Development Program	Columbus, Ohio	1963-66	632	462	Ineffective
9.	Opportunities for Youth Project	Seattle, Wash.	1964-65 (Estimate)	200 (total)		Ineffective
10.	Wincroft Youth Project	Manchester, England	1966-68	54	74	Effective

who have been determined to have serious antisocial propensities but who partake of the service free from official coercion, and 2) have adhered to a research protocol for evaluating service effectiveness. This restricts the inquiry to an assessment of those services given children residing in the open community who voluntarily accept some of the services proffered. Services given children institutionalized for delinquent behavior or probation and parole services can be and have been targets for evaluation. But these services are not "preventive" in a primary sense and are set apart from services voluntarily given and accepted by the factor of mandatory compliance and possible legal sanction against those refusing to comply. Table 1 summarizes the major findings on evaluated delinquency prevention programs.

Except for the Wincroft Youth Study, the delinquency prevention experiments were deemed ineffective; treatment produced no better results than an absence of treatment. This accumulation of negative findings lends powerful support to the contention that little is known about how to prevent delinquency. Wincroft, the one exception to the general rule, is, so far as can be determined, the only delinquency prevention experiment that has been conducted in western Europe. Cultural and societal differences complicate any assessment of this study's applicability to the United States where all other experiments took place.

Regarding evaluative procedures, a prefactory discussion of how this has been done will help to put each project into context. From this, some sense of how valid the assessments of programmatic impact are may be gained. Dilemmas in how to evaluate services, it will be seen, can be as vexing as the dilemmas inherent in providing services themselves. Despite problems with evaluation, the assessment procedures employed in these projects, nonetheless, have about them a quality of astuteness which lends considerable credibility to the evaluation of outcome.

II. EVALUATIVE PROCEDURES

In Table 1, the determinations of the effectiveness or ineffectiveness of the preventive services were taken from the final report of the various projects. These are not after-the-fact assessments or assessments appended by persons unaffiliated with the projects, but rather the results of evaluation mechanisms each project built into its study at the outset and employed to the study's conclusion. What were these mechanisms and why are they superior to other methods of evaluations?

Doubtless, all efforts claiming to prevent delinquency get evaluated in some fashion. Commonly, for example, the accumulated impression of those who give the preventive services and often the testimonies of those consuming these services are offered as evaluative evidence. Unfortunately, while these impressions and testimonies are altogether necessary to underpin speculative inquiry, they are not by themselves able to avoid certain pitfalls. For one thing, firsthand experience frequently results in biased evidence; a desire to see certain results because of dedicated effort is understandable, yet may indeed cloud what actually transpired. But even if seen clear-eyed, the impressions and testimonies of the primary actors suffer from incompleteness. A critical question goes unanswered: Would those who received the preventive service have fared any differently if they had never received the preventive service? Stronger evaluative procedures are called for if this question is to be answered, and the projects reviewed attempted to address that question.

With some variations, the ten projects relied upon the classic experimental design in order to assess programmatic outcomes. Put simply, this evaluative procedure divides into three tasks. First, children or youths considered pre-delinquent are identified and then randomly divided into a treated or experimental group (those who are to receive the service aimed at delinquency prevention) and an untreated or control group (those who will not be exposed to the service). Randomization means only that the assignment of a child to either the treated or untreated group is due to chance alone; this is to guard against a possible biasing effect that could occur if the assignment to treated and untreated groups were done consciously. The second task is to expose those randomly selected for treatment to the experimental service while deliberately withholding that service from those children comprising the control group. Finally, the social careers of the treated group are compared with those of the untreated group; police, court and school records are typically used for making comparisons. The assumption is that significant differences, if any, between the treated and untreated groups are attributable to the services given the treated group because in all other relevant respects the two groups are essentially similar. The question of whether treatment was better than no treatment is answered by reference to the untreated group; the ongoing behavior of this group establishes the behavioral baseline considered normative in the absence of service.

The classic experimental design is not the only way to evaluate treatment effectiveness in a rigorous fashion. In recent years, it has fallen into disuse; the last delinquency prevention experiment discussed here was completed in 1969. More current research has been strongly influenced by social learning theory and the application of behavioral modification techniques. While no attempt is made here to review delinquency prevention studies which relied upon this theory and employed behavioral techniques, a brief review of their distinctive evaluation procedures is useful in understanding discrepant approaches to assessing treatment outcomes and their shortcomings. Typically, the application of behavioral techniques calls initially for a close monitoring of a selected individual's behavior in order to establish the frequency of that individual's antisocial behavior within a given time frame. This before-treatment frequency count serves as the individual's antisocial behavioral baseline against which subsequent behavioral counts are measured. Should the frequency of antisocial behavior lessen significantly during and/or at the close of treatment when compared with the before-treatment frequency count, then it is assumed that the treatment is effective. In essence, then, these projects relied upon a single-subject, before-after evaluation model. A fall in the subject's antisocial rates during and shortly after treatment is taken as an indicator of treatment effectiveness. Each subject serves as his own control, and the question of whether treatment was better than no treatment is answered by reference to the before-after measures.

Both evaluative procedures as described are clear-cut, but in actual practice any number of modifications and compromises in either procedure is possible. The imposition, for example, of the classic experimental design--a design borrowed from the scientist's orderly laboratory--upon the open community with its clutter and disarray is no easy task; consequently, a relaxing or modification of strict procedure is sometimes found. Also, in several of the projects using this design, service strategies ran somewhat counter to the demands of sound evaluative procedures. In two projects, for example, the decision was made to flood a particular geographic area with preventive services and so ruled out the selection of control groups from the areas being served. Comparable youths residing in separate neighborhoods were used as controls. Clearly this compromises the canon which says that the individuals in the target population should be alike in all relevant respects and that his/her position either as a treated or untreated subject occurs by a procedure guided by chance.

How adequate were the untreated groups in serving as benchmarks against which the untreated groups were assessed? The answer to such a question depends in part upon the subjective assessment of the reviewer and reminds us just how qualified our knowledge is in the area.

Similarly, single-subject, before-after designs are not free from flaws. In this design there is an immediacy of "feedback" relating to the subject's behavior which is undeniably useful in modifying the treatment techniques, but serious doubts can be engendered by before-after comparisons. The assumption that a before-treatment measure of antisocial behavior remains constant over time and therefore can legitimately serve as a point of comparison is questionable. Particularly of younger people can it be asked if behavior at Time B (after treatment) should be measured against Time A (before treatment), for the maturation process produces notoriously uneven behavioral manifestations. Some children simply do get "better" and some "worse" for reasons not altogether apparent. Also, larger social events can impinge upon behavior. The declaration of war, the sudden death of a respected civil rights leader, spreading unemployment--these and other events can produce behavioral changes that before-after measures cannot adequately detect and factor out. The behavioral ups and downs of those in a matched control group better reflect the impact of such random factors. Because of this, advocates for the classic experimental design claim that it permits a more valid answer to what the careers of treated may have been had the individuals comprising it never been treated.

The retort has been that earlier behavior cannot be totally divorced from later behavior. There must be considerable assurance that antisocial behavior will persist over time or else it would be impossible to designate any group of children as being "pre-delinquent." This assumption regarding the continuity of behavior must be made even in those studies employing the experimental design; the spontaneous remission of a control group has yet to be reported. Furthermore, not every experimental procedure has to be subjected to the rigors of the classic experimental design in order to demonstrate that the procedure altered behavior. One does not need a control group in order to show that holding a lit match to a bare foot can be a painful experience and that the lit match is the independent variable upon which the sensation of pain is dependent.

Still, it can be countered, delinquent behavior is more subtle and complex than the match-to-foot analogy, and no interventive variable claiming to avert subsequent delinquent behavior has yet been found to equal the self-evident power of the lit match to encourage one to move the foot away from the flame. And however true that those in control groups have demonstrated a persistence to commit deviant acts, the ongoing rate and severity of these acts can be lesser or greater, and these degrees of difference in the aggregate can significantly qualify claims made in behalf of an interventive procedure.

One final observation on a major difference between the two evaluation procedures. By and large, the experiment utilizing the classic experimental design set out to prevent delinquency as delinquency is conventionally defined. That is, they aimed at reducing the rates of juvenile arrest and institutionalization; they often attempted to reduce misbehavior in school as well. Police, juvenile court, and school disciplinary data were collected on the subjects comprising the treated and untreated groups so that these two groups could be compared over time. The official recording of antisocial behavior typically occurs at some interval after the delinquent event itself, while the discrepancies inherent in official reporting usually necessitate a further weighing of official data for the project's evaluative purposes. As a consequence, the rate at which these data are collected and weighed lags considerably behind the rate at which the interventive services are given. Not surprising, then, is the fact that the termination of an interventive service can be followed by weeks, months, or more than a year before the final evaluation of service effectiveness is announced. The classical experimental design permits this disjuncture, and if the divorce of evaluation procedures from interventive procedures has the advantage of placing evaluation upon so broad a data base, it has the disadvantage of not readily informing those providing the service just how they are doing.

By contrast, evaluation and interventive procedures are interdependent in the utilization of behavioral modification techniques. As outlined in the foregoing, a before-treatment baseline is established by an actual count of misbehaviors in a specific, limited time frame. Intervention calls for ongoing frequency counts which are compared with the baseline. These frequency counts usually take place in school and in the home. What is important to understand is that those relying upon behavioral techniques are usually careful to claim that what they wish to alter is the frequency of behavioral problems noted at the time the baseline was established. What was observed when frequency counts were taken may or may not partially coincide with the more conventional and wider-ranging definition of delinquency as that emerges from police, court, and school contacts. In this context, such studies can be viewed as more modest, some would say more realistic, in their intent. Whether or not delinquency is ultimately perceived is largely unknown. Improved behavior in classroom and home have to be viewed as "good" in their own right even if delinquent propensities, as measured by police and court records, remain unaltered.

One final note on estimating the success or failure of a study. Without introducing a discussion of the various statistical techniques used to assess programs' effectiveness, it is enough to know that effectiveness is gauged by the probability with which a particular program outcome could be due to chance alone. Most generally, a treatment program is considered "effective" if in only five cases out of 100 could such an "effective" result be due to pure chance. A five percent level of significance is admittedly arbitrary--why not ten percent or one percent?--but it is a generally agreed upon measure which reduces a chance result to a minimum. When in the reviews a treatment program is said to be "not statistically significant," it is generally this five percent level of significance which is being used.

III. THE REVIEW PROCEDURE

The review of each project will follow a consistent format which includes the following:

1) A background statement which puts the project in some overall context: Where it took place, how it evolved, who funded it, etc.
2) The theoretical orientations which helped guide the kinds of services that were given.
3) The research design employed by the project, and the problems the design may have posed.
4) A statement regarding the treatment providers, who they were and how many were utilized.
5) A statement regarding the treatment population, who comprised it and how persons in this population were identified.
6) Dimensions of treatment, such as the amount of contact treatment providers had with the treatment population, the treatment plan, and the extent to which the treatment population was involved in treatment.
7) The findings of the project.
8) And, in those cases where the reporters made such comment, the recommendations for future undertakings.

In the discussion section following the presentation of each experiment, the review format will continue to be followed. Here, however, the experiments will be brought together so that comparisons, contrasts, dilemmas, and problems can be exposed to view.

This uniform format assures not only a measure of comparability in the discussion of each project that is comprehensive enough so that the reader may gain a genuine sense of each project's dimensions. While most of these projects have been critically discussed here and there in the literature, nowhere have they been brought together and described sufficiently for an understandable composite to emerge. It is hoped, therefore, that this presentation can serve as a unique and convenient resource for program planners and evaluators. The projects are presented in chronological order.

IV. STUDIES

The Cambridge-Somerville Youth Study, Cambridge and Somerville, Massachusetts

(Powers and Witmer, 1951; subsequent parenthetical references in this section, unless otherwise noted, are to this work).

A. Background

The prototypic delinquency prevention experiment, the Cambridge-Somerville Youth Study, was conceived and funded by a wealthy physician and educator, Dr. Richard Clark Cabot of Harvard. He had been impressed by the work of Shelton and Eleanor T. Glueck, whose book, *500 Criminal Careers*, helped convince him that lives of crime began early in disruptive home life. Cabot came to believe that the intervention of a friendly, socialized adult with a vulnerable child when the child was still quite young (six or seven years of age) might lead the child to a normal, non-delinquent life. In July, 1935, with his own funds, he established a non-profit foundation in the name of his late wife, the Ella Lyman Cabot Foundation; this charitable corporation sustained the delinquency prevention study and project from its inception in 1935 to its close in 1945.

The project was unaffiliated with any youth-serving agency; it was brought into existence for the purposes of research and ceased to exist once the research was done.
The study fell into two parts, a start-up phase running roughly from 1935 to 1938 when it was conceptualized, staff was employed

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and boys identified; and the service phase running from 1938 through 1945 when the treatment was given. Research was a part of the entire study and carried on for several years once the service ended in order to determine how the boys had done socially.

B. Theoretical Orientation

Dr. Cabot evolved no elaborate theoretical concept of delinquent behavior or of how such behavior could be prevented. He believed that 'moral suasion' as exemplified by a friendly, concerned adult of good character would prove decisive in the development of a boy who otherwise was delinquency prone. In orienting the treatment staff to his view of treatment, he stressed, "The first fact about our growth is our dependence on God" (p. 94). Spiritual growth should be an essential part of treatment. In selecting treatment staff, Dr. Cabot did not recruit only those coming from a particular profession, such as social work or clinical psychology, nor did he insist that the staff members subscribe to even a generally agreed upon theoretical framework of treatment. The bulk of the work, Dr. Cabot said, would be accomplished through the 'personal intimacy' of workers with boys and their families (p. 95). A warm, friendly adult serving as a role model and sticking with a boy for a prolonged period of time in order to share the good and the bad summarizes Cabot's view of how delinquency was to be prevented.

C. Research Design

From the outset, it was understood the treatment program would be assessed for effectiveness through utilization of the classic experimental design. However, without preceding experiments to guide them, the project staff had to modify procedures as they went along. The design called for the identification of 'bad' boys (girls were deliberately excluded from consideration) approximately 5 through 9 years of age. Teachers of kindergarten through third grade in the Cambridge-Somerville public schools, along with the school principals, were used as primary sources of referral. The project supplied the teachers with a list of antisocial behavior (persistent truancy, stealing, cheating, etc.) (p. 30) which was to help teachers in making nominations. When teachers proved reluctant to designate 'bad' boys, the research design was altered so that 'average' boys whom teachers considered in need of social services could also be referred; as a result, referrals of both kinds came in at a much faster rate. To a lesser extent, referrals were accepted from 1937 through the Spring of 1938, by which time 1,953 names had been collected. An elaborate procedure for generating information on each referred boy was instituted. Home visits were made with interviewers using a uniform schedule to rate the home condition on such items as standard of living, discipline by parents, mother's personality, etc. (p. 41) Teachers were again asked to rate boys on such behaviors as "troublemaking, aggresiveness, undesirable habits, etc." (p. 44). IQ tests were administered; physical examinations given; teachers were interviewed individually. Neighborhoods in which the boys lived were given delinquency ratings. Information on each dimension was not generated for each of the boys, but each boy was studied sufficiently that a "profile" emerged which gave a glimpse of the boy's family, the boy and the boy's neighborhood environment.

The decision was made to restrict the study to 650 boys, with 325 serving as experimentals, 325 as controls. A three-person selection committee--a psychiatrist; the director of casework at The Massachusetts Reformatory; the head social worker for the Division of Classification, State Department of Correction--rated each boy separately, with the ratings then combined to determine the agreement among the raters. In 782 cases, there was high agreement among the raters regarding the classification of these boys. An elaborate procedure was then employed to "pair" boys. Rating each boy on 19 variables (such as health, IQ, home, neighborhood, etc.), it was possible to identify those two boys who most closely resembled one another; a coin toss determined which one of the pair would serve as the experimental, and which the control. In this manner the 325 pairs were ultimately made and assigned to groups. For "public relations" reasons, 42% of all boys selected were 'average' boys (that is, were not considered to be highly delinquent) whereas the remainder were 'bad' boys.

D. Treatment Providers

Dr. Cabot took an active interest in overseeing the project during its formative years. Upon Dr. Cabot's death in 1939, the project's co-director, Dr. P.S. deQ. Cabot, a psychologist, assumed the directorship until 1941, when he relinquished that post to Edwin Powers who had been one of the first counselors hired. Powers' academic discipline is not stated. Over the life of the project, nineteen different counselors were employed, with no more than ten working at any one time. Fifteen were men, four women; eight were professional social workers; six had completed part of the academic requirements for a degree in social work; two were experienced boys' workers; one was a trained nurse; and two were psychologists. While social workers predominated, professional identification was not central to being a project counselor: "A social worker, no matter how well trained was not to be preferred to a warm, outgoing person who had that vital spark so essential in human relationships. . ." (pp. 92-93).

E. Treatment Population

The experimental boys at the outset of the treatment program ranged in age from five years three months to twelve years eight months, with average being approximately eleven years. IQ testing placed the average intelligence quotient for the experimental boys between 92 and 96. Twenty-seven (27) experimentals were black; in only about one-third of the cases (117) were the parents born in the United States. Only seven boys had prior records with the juvenile court. Forty-five (45) boys came from homes with one or both parents missing. Most of the experimental boys (220) came from the industrial sections of Cambridge; Somerville, contiguous with Cambridge, was somewhat better off economically than Cambridge and appeared to have less of a crime problem.

F. Dimensions of Treatment

(1) Contact Time

The study did not routinely keep track of contacts made with or on behalf of an experimental boy; such information was part of the discursive prose record kept on each boy, but in that form, was not readily available. A sample of 60 representative cases revealed that during the first year of treatment, all visits with or on behalf of a boy averaged 33.7, with the boy seen alone comprising 4.1 of those visits and with family conferences comprising 11.6, the greatest number in any one category of visiting. During 1940, counselors kept a tabulation of all visits for that year. Contacts with or on behalf of the 322 boys then in the study numbered 8,804, or an average of 27.3 per boy (pp. 124-126). Overall, it appears that on the average all contacts with and for a particular boy numbered fewer than three a month.

(2) Treatment Plan

The first experimental boy was contacted in December, 1937; in 1938, 71 boys were engaged; 91 more were added in the first four months of 1939; the last 158 boys were reached in May, 1939. By the latter date, the treatment staff reached ten counselors, so that each counselor was responsible for approximately 32 boys. The original treatment plan, as conceived by Dr. Cabot, was to have service continued for approximately ten years, or roughly the time at which a boy would enter public school at 5 or 6 and continuing on through the boy's mid-adolescence. The delay in finding and selecting the study population on the one hand and the onset of World War II on the other tended to compress the actual time treatment was given largely within a four-to-five-year period (1939-42), with all service terminated to the last 75 boys in December 1945 (pp. 152-153).

From 1939 to 1941, the program was administered from a small house in Cambridge; counselors worked essentially in the field, making home visits, seeing teachers, taking boys on trips, arranging for referrals to other agencies and services, etc. The treated boys were never explicitly told why they were the objects of interest. Few boys and their families made probing inquiries concerning the counselors' intent; the endorsement of the program by the public and parochial schools seemed to make it acceptable to boys and parents (pp. 152-153). In only one case was there an outright refusal to participate; suspicion and indifference characterized about 41% of the selected cases but did not seriously deter entry into service.

Once in service, the boys and their parents were individualized, so that no treated boy was likely to know many other boys in the program. The counselors were diverse in their views and treatment beliefs; some favored "psychological techniques," others "physical development," some gave considerable attention to "family problems," some to "school adjustment" or "religious training," and some "were not conscious of any particular emphasis" (p. 115). "Friendliness" appeared to be the one attribute which most closely linked all counselors in their treatment approaches.

In 1941, the program was reorganized; the program moved to a larger, refurbished house which could accommodate a private interviewing room, a woodworking shop in the basement, and some recreational activities on the lawn. To some extent, the program became more building-focused. Some counselors wanted the program to become recreationally oriented and some small groups of experimental boys were started. Nonetheless, the staff decided not to alter drastically the original treatment emphasis: "casework with the individual boy" (p. 130). Overall, the counselors appeared to strive for particular objectives, most notably the boys' school adjustment and the provision of tutorial services; the boys' physical well-being, particularly at the outset of service when boys were frequently taken for medical, dental and psychiatric services; the provision of a summer camping experience; the remediation of such family problems as unemployment or illness of the parent (and in rare cases, supplying financial aid); and in 24 cases, finding foster homes for boys whose own homes were inadequate" (pp. 116-118, 131-134). Beginning in 1941, a psychiatrist was employed one afternoon a week to consult with counselors about particular cases.

(3) Involvement

The largest number of 'average' boys were in time perceived by the counselors as needing little service, and in fact, took time away from the more difficult cases. Starting in 1941, a process of "retiring" cases was initiated, so that by summer 1942, the number of treated subjects dropped to 257 with the less difficult boys being terminated. By 1943, the war was making the treatment of older boys difficult, so that boys reaching 17 were automatically terminated. The experimentals decreased from 274 in 1941 to 75 in 1945 when the project closed.

In 1940, when an effort was made to assess the boys' receptivity to the treatment program, it was determined that in only 22 out of 322 active cases (7%) was there unwillingness or a lack of cooperation in client participation. Another 50 cases (15%) were inactive because counselors did not consider these boys in need of service.

G. Findings

An elaborate evaluative scheme was used to assess many facets of the experimental boys' behavior and beliefs, such as emotional maturity, sense of altruism, home adjustment, and the like. Not all of these dimensions will be reported here; the more central concerns relating to delinquency, such as police arrests, court appearances and commitments to institutions for delinquents, will be elaborated.

The names of experimental and control boys were checked against Cambridge police files from 1938 through 1946 (one year after service ended). During this time, 114 experimental and 101 control boys became known to the police; 49 boys in each group had one contact with the police, while 65 experimentals and 52 controls had two or more contacts. "Throughout the treatment period the counselors were evidently not successful in preventing boys from committing offenses that brought them to the attention of the police in Cambridge. . ." (p. 325). Regarding court appearances, all boys were cleared through state-kept probation files as of July 1948; these were central files noting state and county court appearances. The names of 96 experimental boys charged with committing 246 offenses appeared, whereas 92 control boys with 218 offenses appeared. Commitments to state juvenile correctional institutions as of

November 1948 revealed 23 experimentals and 22 controls had been committed. One encouraging note on commitments to institutions was that more control boys (15) than experimentals (8) were committed to institutions for older offenders. From these findings, the research staff drew the following conclusions: "The special work of the counselors was no more effective than the usual forces in a community in preventing boys from committing delinquent acts," and ". . .though the first stages of delinquency are not wholly averted when starting treatment at the eight-to-ten-year level, the later and more serious stages may to some degree be curtailed" (p. 337).

H. Recommendations

This study was evaluated by a professional psychiatric social worker who was critical of the study's treatment philosophy (p. 341-583). In essence, this evaluation, following upon the evaluation of outcome by the research staff, concludes that the study embraced a good number of boys who either did not need help or were so "seriously neurotic" that the services given were inappropriate. For the kind of service given, delinquency as a target for change should not be the goal; identifiable problems, such as health, home and school problems having definable dimensions, can be successfully attacked by boys and parents who are willing to solve problems but lack the resources and skills which a good counselor might provide.

New York City Youth Board, Validation Study of the Glueck Prediction Table (Glueck and Glueck, 1950; subsequent parenthetical references in this section, unless otherwise noted, are to this work.)

A. Background

In the late 1940's, the lifetime study of delinquent and criminal careers by two Harvard criminologists, Sheldon and Eleanor Glueck, culminated in their construction of a widely heralded delinquency prediction device, the Glueck Social Prediction Table (Craig and Furst, 1965:165-171). To test the validity of this prediction table, it was adopted in the early 1950's by the New York City Youth Board (NYCYB), with financial assistance from the Ford Foundation, as the basis for an experimental study. The NYCYB first used the table in 1952 by applying it to all boys, overwhelmingly comprised of minorities, then entering the first grade in two elementary schools situated in high delinquency areas of New York City. In addition to obtaining prediction scores on better than 200 boys, the research design called for providing interventive treatment of a sample of those boys whose delinquency potential was found to be in the moderate to high range. Treatment was to be given to 29 experimental subjects through a child guidance clinic in one of the two schools; 29 nontreated control subjects attended the second elementary school.

B. Theoretical Orientation

The Glueck Table reflected the view that disorganized family life alone was the genesis of delinquent behavior. Five factors composed the table: (1) discipline of boy by father; (2) supervision of boy by mother; (3) affection of father for boy; (4) affection of mother for boy; and (5) cohesiveness of family. In practice, the delinquency prevention table frequently had to be collapsed because of the absence of a father in the home; "mother's discipline" then substituted for the two-father factors. A family evaluation by a person trained to use the table generated scores on the various factors, with the higher cumulative score being seen as an indicator of higher delinquent potential.

Treatment appears to have been guided by theoretical tenets of ego-psychology. While the particulars of treatment were not given nor a theoretical orientation explicitly stated, treatment appears to have been individually or family oriented and to have been guided by the tenets of ego-psychology. The treatment was described as "psychiatric and reaching-out social work" and as "child guidance therapy." The importance of familial relationships, the affective attributes of subjects and the use of terms such as "established psychiatric practice" strongly suggests a reliance upon ego-psychological theory.

C. Research Design

Boys to be treated were drawn from one of the two schools, while boys in the second school served as untreated controls.

In the first school, twenty-two boys were identified as generating moderate (13 boys) to high (9 boys) prediction scores on the Glueck table. An additional seven boys, who had not obtained high prediction scores, were put in the experimental group through teacher nomination. No explanation is given why these seven boys were added. From the second school came the untreated control boys who were matched with their experimental counterparts on the basis of neighborhood, prediction score, ethnic group, age, and I.Q. score. As a result, twenty-nine experimentals were matched with twenty-nine controls.

D. Treatment Providers

The child guidance staff providing the interventive treatment are identified as "psychologists, psychiatrists, and psychiatric social workers" (p.168). All had a minimum of three years professional work experience. Neither the number of treatment providers nor the size of the providers' workloads is given.

E. Treatment Population

While the target population from which experimentals and controls were drawn was comprised predominantly of minorities (131 blacks, 40 Puerto Ricans, 53 whites), the racial composition of the 58 experimental and control group is not given. The boys selected were approximately six years of age at the time treatment was initiated; it is assumed that blacks and Puerto Ricans were heavily represented.

F. Dimensions of Treatment

(1) Control Time

From start to finish, the treatment span covered five years (1952-1957), with the median length of staff contact with experimental boys being fifty months (24 boys). The range of contact time was from 16-19 months to 60 months and over. The precise number of contacts the experimental boys and their families had with the treatment providers is not given.

(2) Treatment Plan

No clear picture of the child guidance treatment regimen is provided. It does appear that most of the treatment was given in the school-based clinic, but that this was supplemented with visits to the homes of the experimental boys. A distinction is made between "therapy" and "concrete services." For example, many mothers were said to accept "concrete services"--that is, camp placements and keep in crisis situations--but few wanted "clinical help" for their children.

(3) Involvement

The mothers of the nine boys with highest probability scores were never engaged in the treatment process. Of the mothers of the thirteen boys having more moderate scores, none was considered truly receptive to the clinic's help, but "eight tolerated it and were willing to involve themselves to a limited degree in the treatment process" (p. 168). The seven teacher nominees, diagnosed as having at least average intelligence and exhibiting "behavior disorders" and "neurotic traits," (p. 168) had mothers who were cooperative and "made good use of (the clinic's) services" (p. 169). Just how engaged the young experimental boys were in the treatment process is not stated.

G. Evaluation of Effectiveness

"After approximately five years of psychiatric and reaching-out social work (and several years after termination of such therapy)....the same number of serious delinquents appeared in each group" (p. 170). Ten boys in each group acquired delinquent status for a range of offenses from shoplifting to larceny of motor vehicles. Treated boys, however, appeared to commit delinquent acts at a somewhat later age than did the controls. The groups were alike as well on behavior judged "troublesome" but not delinquent. The project reporters conclude: "This study offers no encouragement for the hope that child guidance therapy offers a means of materially reducing the incidence of serious delinquency in a population of boys selected by the Glueck Social Prediction Table as probably delinquents."

H. Recommendations

The project's reporters suggested that perhaps prekindergarten classes in high delinquency areas may be helpful. On the other hand, they also suggested that the early adolescent years may be more promising for therapy on the supposition that children then may be ready to assume more responsible social roles.

Maximum Benefits Project, Washington, D.C. (Tait and Hodges, 1962; subsequent parenthetical references in this section, unless otherwise noted, are to this work.)

A. Background

The Maximum Benefits Project was an outgrowth of Washington, D.C.'s Youth Council. Established in 1953, the Council was charged with the responsibility of dealing with the overall problem of delinquency within the District. The Council's governing board embraced a wide range of civic and political leaders--the Superintendent of Schools, the Chief of Police, the Judge of the Juvenile Court, etc.--who wished to develop a delinquency prevention program centered in the public schools. The Project began in 1954 being funded initially by contributions from governmental departments and community agencies, and starting in 1955, was funded by the Eugene and Agnes E. Meyer Foundation. Two elementary schools in the most deprived section of the city were selected as the sites for the experiment.

B. Theoretical Orientation

This project, like the New York City Youth Board's Study (pp. 25-9), saw the family as the focus of delinquency. While no elaborate theoretical position is explicitly stated, the project directors utilized the Glueck Prediction Table with its exclusive focus upon parent-child interrelationships as the crucial variable. Additionally, they relied upon the earlier psychiatric orientation devised by Healy and Bronner, who also held that "the most immediate, most influential, and most conditioning environment of young human beings is that of family life and its relationships" (Healy and Bronner, 1936:141). The project's service was loosely said to be "concentrated on the case work approach" with a psychodynamic emphasis (p. 27, 62-63). Psychiatric classification terms were frequently used to describe the study population--"transient situational disorders," "personality disorders," "psychoneuroses and psychophysiologic disorders," etc.

C. Research Design

The project ran for three years (1954-57). The experiment called for school personnel in the two selected elementary schools to make referrals of delinquent-prone children to the project. Those referred would receive from the project staff an "initial case study" consisting of a home visit by a social worker, an investigation by a nurse of the health conditions of child, home and family, some psychological testing and a psychiatric interview of child and parent. Following the initial study, the Glueck Prediction Table was used in order to generate a delinquency-prone score, with children having the higher scores being considered prime candidates for inclusion in the project's study. At that point, children were assigned either to the treatment group or to the non-treatment (control) group. Not explicitly stated is how this assignment was made. Over the life of the project, 111 children were assigned to the treatment group, 68 to the control group. A comparison of those treated and non-treated children in the first year of the project

(1954-55) showed the two goups to be "not grossly dissimilar although not as closely comparable as had been hoped, largely because transient habits of certain families made it difficult to the plan of matching them in terms of age, race, sex, and Glueck scores" (p. 56). If anything, the first controls in terms of age, grade attainment and Glueck scores could be considered slightly more delinquency prone than the treated group.

The project accepted both boys and girls, and in grade levels from kindergarten through sixth grade (age 5-12 years). No comparative data were provided showing how all 111 treated subjects matched with the 68 controls on such variables as race, sex, age, grade, I.Q., etc. Such data were provided only for those in the project's first year (treated group numbered 37, untreated 32) (Table 7-3, p. 57). This showed the treated group to be comprised of 7 white males, 24 non-white (predominantly black) males, 6 non-white females; the group's average age was 7.5 years.

In addition to the provision of an interventive service, the project wished to assess the accuracy of the Glueck Prediction Table in identifying young children who would at a later age become officially adjudicated delinquent.

D. Treatment Providers

In addition to the psychiatrist in overall charge of treatment, the staff included two full-time social workers, two part-time psychiatrists, one part-time clinical psychologist, five graduate students (believed to be in graduate social work programs). The treatment staff was decidedly professional in composition.

E. Treatment Population

The final report does not provide information which identifies all persons who received the project's services; such data were provided only on those who were engaged during the first year of the project's life. The project's theoretical orientation pinpointed the family as the target for service, so that in addition to the children in the experimental group, parents and significant others were also perceived as candidates for service. The initial 37 experimental children came from 32 families (five families contributed two children each). In these 32 families, 28 mothers, 7 fathers, 5 grandmothers, 2 mother surrogates and 9 adult "others" (such as housekeepers and parental paramours) were recipients of service. It is evident that while the children were seen as the "criterion" service population whose behavior would be monitored in order to determine service effectiveness, the actual treatment population was much broader with significant others perceived in some cases as being more appropriate for service than the children themselves.

F. Dimensions of Treatment

(1) Contact Time

Again with reference to only the first 37 experimental children, treatment services were given from a

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minimum of two months to a maximum of 35 months, with the average being 19 months. On the average, a child had 12.1 interviews with the treatment providers; the parent, when engaged, 11.4 interviews. Interviews with children were brief, 20 minutes on the average; interviews with adults lasted from 45 to 60 minutes (p. 62).

Working from these averages, it can be estimated that all interviews with or on behalf of a particular child numbered 23.5 and that the interviews conducted during a 19-month period amounted to 1.3 interviews a month, lasting approximately 52 minutes in all. These computations are not found in the final report, nor can they be generalized with assurance to the entire 111 experimental children, for 64 of whom no treatment data are provided; but with these reservations in mind, it appears treatment was not intensive in terms of time.

(2) Treatment Plan

A child selected for service became the subject for a staff planning conference in which an individualized treatment procedure was evolved. In addition to social casework services, the treatment plan could include making suggestions to the child's teachers and seeking outside services such as legal aid, psychotherapy, Big Brothers, placement of child in foster care and "other." Other than the making of suggestions to teachers regarding possible management of the child, the core of the services actually provided was casework; attempts to refer children and their families to outside services were considered in the main unsuccessful.

(3) Involvement

While the context of treatment was psychiatric and psychoanalytic, the project staff were aware of the disjuncture between theory and the project's services. In traditional psychoanalytic practice, "the patient requests services and comes to a quiet office several times a week on a regular schedule" (p. 62). In contrast, the project's selected experimental population and their families exhibited no instance when they initiated a request for aid, did not come to a quiet office but were usually seen at home, often "in the presence of noisy children," and in only a small number of cases followed a regular schedule, with irregular crisis management being the rule. "The majority of families became either actively or passively uncooperative," with one family throwing dishes at the staff social worker during a home visit (p. 72). In most instances, the families did not carry through with the casework plan because of decreasing or limited cooperation.

Referrals to other services were largely a failure because neither the families nor the outside agencies were receptive to one another (p. 69).

G. Findings

A follow-up in 1958 of all 111 treated subjects and of 64 of the control subjects revealed that 42 treated subjects

(39% of the treatment group) had acquired police or court records while 14 untreated subjects (25% of the control group) had records. The evaluators concluded that "within the project definitions . . .we failed to demonstrate that project 'treatment' of these elementary school children 'prevented' delinquency; i.e., reduced the incidence of their later delinquencies as measured by court and police experience" (p. 64).

A secondary evaluation goal was determining the accuracy of the Glueck Prediction Table in identifying future adjudicated delinquents. By 1958, 32.8% of those predicted to become delinquent had become known to the police and juvenile court (p. 51). The population for which predictions were generated was still quite young in 1958--13 years of age or younger--so that the accuracy of the Glueck Table could not be properly assessed. So far as is known, no final assessment was ever published or perhaps made. All members of the target population would have to reach 18 years of age before prediction accuracy could be determined. The subsequent careers of those boys in the control group would be of particular interest, for being untouched by the project's services, the controls could be assumed to reflect with greater accuracy the true power of the prediction device.

The accuracy of prediction devices was an intimate concern of two other projects. The New York City Youth Board's project also relied upon the Glueck Table while the staff of the Youth Development Project of Columbus, Ohio developed its own prediction device. In the discussion following the review of individual projects, the problems posed in attempting to make early predictions of future delinquents will be elaborated upon; it should be evident, however, that mispredictions and the possibility of being falsely labeled "pre-delinquent" can be viewed as a serious erosion of an individual's civil liberty. All "delinquency prevention" programs attempt to predict in some manner, and so all delinquency prevention efforts must face squarely the issues posed by failure of their prediction methods.

H. Recommendations

The failure of the project's services prompted the staff to speculate at length about what would be needed to serve "incompetent families." They proposed setting up within the broader community a 'therapeutic sub-community' or 'family hospital' which would be "under the direction of specialized personnel and would provide a more structured environment for the children and at the same time keep them with their families" (p. 146). In addition to insuring that the basic needs of children were met, there would be "a more explicit use of psychodynamic principles in the operation of such a sub-community" (p. 146). Not stated was what kind of legal status would be conferred upon persons in such a community nor was there discussion of the cost of operating such a sub-community.

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The Midcity Project: Boston, Massachusetts (Miller, 1957; subsequent parenthetical references in this section, unless otherwise noted, are to this work.)

A. Background

The Midcity Project grew out of community concern following the murder of a rabbi allegedly by a gang of black youths. The murder touched off a series of newspaper articles which dealt with gang delinquency in Boston. In 1953, United Community Services, Boston's central agency for planning, financing, and coordinating private health and welfare services, moved to set up a special committee charged with studying the problem of delinquency in Boston and proposing a means for addressing the problem. The special commitee brought forth the recommendation that a three-year delinquency prevention experimental program with an evaluation component be initiated in the district where the rabbi's murder took place; the program was to employ multiple, coordinated preventive measures intensively within an area containing approximately 30,000 people. Administratively, the program was not part of any existing social service agency, so that during the project's existence, the program constituted a relatively separate and autonomous agency. Funding came from private local trust funds and from United Community Services. The project was initiated in June 1954 and terminated in June 1957 (p. 390-391).

B. Theoretical Orientation

The predominant service to be given in the project was modeled after the detached street worker approach pioneered by the New York Youth Board in the early 1950's. While it appears that this methodology evolved no formal theoretical position, it assumed that "gang members were essentially isolated within their own adolescent slum world and [either were] denied or lacked the ability to seek out 'access' to major adult institutions." The Midcity project attempted to "open up channels of acces to adult institutions--particularly in the areas of education and employment" (p. 173). The belief was that "environmental manipulation" was more likely to reduce delinquency than "personality change."

Treating the personality was not discounted; most service providers had prior training involving "the principles of... psychodynamic psychotherapy" (p. 174). However, the "corner gang world" was not perceived as a conducive environment for psychotherapeutic techniques which were to be utilized "through indirect rather than direct means" (p. 174).

C. Research Design

Because the delinquency prevention services were largely focused upon preadolescent and adolescent street corner gangs found in a lower-class section of Boston, with memberships in these gangs potentially accessible to any child living in the service area, the classic experimental design could not be employed in assessing service effectiveness. Instead of randomly dividing a designated pool of delinquency-prone children, all of whom resided in the same area, into experimental and control groups, the Midcity researchers used essentially two alternate methods for determining service impact.

First, the trend in illegal acts as established by gang members in their appearances before juvenile court prior, during, and after the receipt of services were analyzed. This was to help answer the question of whether there was a decrease in the frequency of statute violations committed by the service recipients during the period of exposure to service.

Secondly, a control group comparison was considered necessary in order to overcome the shortcomings inherent in a comparison of before-during-and-after trends, most notably the difficulty of ascertaining whether such trends were significantly different from the trends established by delinquency-prone children who had not received the service. During the course of the project eleven street corner gangs comprised of 172 individuals, who were reported to be similar to the experimental subjects in terms of age, sex, ethnicity, social status, and area of residence, were identified as not having received project services. These eleven non-served gangs were treated as controls for the 21 corner gangs which did receive service.

In addition to these two methods for assessing, the research staff devised several other measures for evaluating service, such as a method for assessing trends in disapproved

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behavior as distinct from law-violating behavior. The other measures will not be discussed here because they did not as directly address the question of how effective was the service in reducing delinquent behavior.

The project was set up as a three-year demonstration project (June 1954 through May 1957). From what is reported, it appears no precise determination was made prior to the onset of service how many gangs or individuals would receive service and in what amounts. As it turned out, 21 gangs comprised of 400 youths ultimately received service in varying amounts; the duration of service contact with the groups ranged from 10 to 34 months.

D. Treatment Providers

During the life of the project, seven direct service providers were employed; five were men, two women; all were "professionally trained, with degrees in case work, group work, or both" (p. 169). The project was directed by a professional anthropologist having a doctorate degree.

E. Treatment Population

During the life of the project, 400 youths between the ages of 12 and 21, comprising the membership of 21 street gangs, were served in some form. Of this total, seven gangs, comprised of 205 individuals, received "intensive attention," and it is these seven gangs which constituted the experimental population used for assessing service impact. Five of the gangs, having an average membership of 30, were male; two, with an average membership of nine, were female. Four gangs were made up of white males; one made up of black males; one of black females; one of white females. No specific counts of the gang members' racial and sexual identities were provided. It can be seen that the total (N=168) derived from the averages given the gang size for males and females (30x5 and 9x2) falls considerably short of total number of individuals (N=205) said to have been served. This discrepancy was nowhere reconciled.

Nineteen of the gangs were found in "lower class" neighborhoods" which the project researchers defined as neighborhods in which 50% or more of the adults had failed to finish high school and 60% of the male residents pursued occupations in the bottom five occupational categories as found in the census of 1950. The remaining two gangs were in "middle class" neighborhoods. Precisely how many of the served gang members came from families having lower or middle class characteristics was not given.

F. Dimensions of Treatment

(1) Contact Time

It was reported that the service providers "contacted their groups on an average of 3.5 times a week; contact period averaged about five or six hours; total duration of contact ranged from 10 to 34 months" (p. 170). This statement is somewhat ambiguous; it could be interpreted that each group was seen on the average of 3.5 times per week by a service provider or that on the average the service provider met a total 3.5 times per week with assigned groups. Also unclear is how much service individuals received on the average; because group membership and an individual's attendance in a group fluctuates, it cannot be safely assumed that the number of contacts with a group closely approximates the number of times a particular group member was exposed to the service provider. On the surface, the service appears to have been "intensive," but, as presented, the evidence leaves room for doubt.

(2) Treatment Plans

The project's final report enumerated four treatment strategies which were said to be employed: (1) A community program aimed at strengthening local citizens' groups that they might address local problems such as delinquency; (2) an effort to coordinate the community's professional service agencies so that a cooperative and unified approach to the problem of delinquency might evolve; (3) a casework program for families having histories of long and troubled relationships with public welfare agencies; and (4) street work with gangs comprised of young people characterized as the "major effort of the project" (p. 169). The final report does not elaborate on any of the treatment approaches other than work with the gangs, nor were these other approaches evaluated in determining service effectiveness.

The overall treatment framework for working with the gangs was thought out in advance. The service providers were to try to change the informal organizational system of the street corner gang to a more disciplined, formal organizational system which would allow for the conscious setting of lawfully-oriented activities. It was assumed that much gang delinquency sprang in an unplanned way out of idleness and a sense of boredom. Secondly, it was assumed that lower class gang members were isolated from the "major adult institutions," instead having access to "illegitimate" activities, such as "thieving, fighting, and prostitution" (p. 173). The service providers therefore attempted to cultivate opportunities in the areas of education and employment. Thirdly, gang members were seen as being poorly served as they interfaced with outside institutions generally. The service providers would act as brokers and advocates for gang members, arranging for lawyers to represent members in court, interceding with judges and parole officers, obtaining the use of outside facilities such as dance halls and gyms. And finally, in the project's later service phase, it was assumed that the gangs, once formally organized, could join with the local citizens' groups comprised of adults, whom the project had also organized in order to enhance the achievement of community betterment and reinforce the youths in their new roles as non-norm-violating club members. While "personality change" through the use of psychiatric techniques was not primary treatment strategy, the service providers did consult regularly with a psychoanalytically trained psychiatrist and had available the services of child-psychiatric clinic and family-service agencies if they considered these services as being needed by and acceptable to particular gang members and their families.

(3) Involvement

No mention is made in the final report of how involved in the project the gang members were believed to be.

G. Findings

Trend data and the comparison of experimental and control groups were used to assess service impact. Trends of illegal behavior were established by being based upon the frequency of delinquent acts as observed by the service providers and based upon court appearances. During the service phase of the project, providers recorded 1,008 legally prohibited acts; 80% of 205 gang members contributed toward this total. When the delinquent acts were classified for seriousness and grouped into three corrective time periods, "it could not be said that there was any significant reduction in the frequency of known crimes during the course of the project" (p. 181). "Trends in court-appeared offenses were essentially the same as trends in illegal actions" (p. 183).

Trends, while useful, could not answer a crucial question: Were these "trends related to the workers' efforts"? This question could not be answered without a control group used for comparative purposes. From what was reported, it appears the effort to construct a control group occurred at the close of or after the delivery of service. The project staff recognized that post hoc, non-random matching of contols and experimentals posed "risks--primarily the possibility that service and control populations might not be adequately matched in some respects" (p. 185). With this reservation, the project researchers identified 11 corner gangs comprised of 172 members who received no project services but who in terms of age, sex, ethnicity, and social status approximated the 205 youths who did receive service. When compared, "the court appearance frequency curves for project and control groups were very similar" (p. 185). Also, the numbers of individuals in each group appearing and reappearing in court were comparable. "All major measures of violative behavior--disapproved actions, illegal actions, during-contact court appearances, before-during-after appearances, and Project-Control group appearances--provide consistent support for a fighting of 'negligible impact'" (p. 187).

While not rigorously evaluated, project goals in other areas were said to have been reached. The project claimed to have a "calming effect on the adult community," and was "instrumental in establishing new delinquency-control organizations" (p. 189).

Youth Consultation Service Project, New York, New York

(Meyer, Borgatta, and Jones, 1965; subsequent parenthetical references in this section, unless otherwise noted, are to this work.)

A. Background

Youth Consultation Service (YCS), a well established, nonsectarian, voluntary social work agency in New York City, specialized in providing predominantly casework services to adolescent girls. In the early 1950's YCS became concerned about older high school aged girls who as a result of persistent school difficulties were referred to the agency for counseling service, but who either did not make contact with the agency or, if accepting the referral, exhibited such "severe stages of maladjustment and psychological pathology" that they could not be appreciably helped. The YCS staff thought that if the girls could have been reached earlier, perhaps at outset of their high school careers, referrals and treatment might have been more successful. The agency established a Research Committee, comprised of social workers and social scientists, in order to formulate a research proposal that would concretize the notion of a preventive program for younger high school girls. The specific proposal that emerged was funded by the Russell Sage Foundation and the Grant Foundation. The actual research project began in the fall of 1955 and ended in the spring of 1960.

B. Theoretical Orientation

The treatment staff of YCS appears to have subscribed to the tenets of ego-psychology. Nowhere is the agency's theoretical orientation explicitly elaborated, but the use throughout of traditional psychiatric terminology--"neurotic personality," "character disorder," "poor ego strength," "psychotic-like behavior," etc.--and the reliance of treatment staff upon psychiatric consultation make it reasonable to assume that YCS staff saw itself providing remedial services to girls with damaged personalities. "Neurotic symptoms and behavior disorders due to neglect were thought to account for many of their (i.e., the girls') weaknesses," is an example of how one worker described the overall problem of a particular group of girls (p. 135).

C. Research Design

Girls entering a vocational high school situated near the social agency were seen as an accessible and appropriate group from which these girls showing potential social problems could be selected. Those girls predicted to have increasing difficulty as they moved through school would comprise a pool of subjects which through a randomization procedure could be divided into experimental and control groups. The design called for a rating by the research staff of girls entering the vocational high school during the fall. These ratings would be conducted for four consecutive years, starting in the fall of 1955. Consequently four cohorts, or pools, of girls were identified (fall 1955, 1956, 1957, 1958). All together the four cohorts provided the project with 189 experimental subjects and 192 control subjects. (About one-fourth of the approximately 500 girls entering the school each year were rated as showing potential for having serious social problems.)

The actual selection procedure used by the research staff involved an examination of each entering girl's cumulative school record. This permitted the researchers to judge a girl's school behavior, personal characteristics, and to a limited extent the home situation. The researchers deliberately chose not to administer psychological tests or otherwise introduce procedures which generated information in addition to that found in the extant school records; they were convinced that the behavioral and social information routinely noted by teachers and counselors was sufficient to identify girls likely to have increasing difficulty.

Following assignment to an experimental group, a girl was invited to become a recipient of YCS service. Although nowhere in the report is it stated precisely how long service lasted, it appears that the term of service for each experimental cohort was a minimum of one school year; in one place, the report speaks of girls "who had been in contact with YCS for several years" (p. 47). School performance and behavior were routinely assessed as was such out-of-school behavior as out-of-wedlock pregnancy and being known to the juvenile authorities. Additionally, the research staff devised several questionnaires designed to measure behavioral and social attitudes and to elicit a girl's plans for the future. These were administered to both experimentals and controls once service began and so allowed for comparisons to be made.

In all, four cohorts of experimental and control subjects were selected; the first three cohorts had at least three years at the vocational high school before the project closed, while the final cohort had two years (fall 1958 through spring 1960).

D. Treatment Providers

The treatment providers were professional social workers, all of whom were grounded in casework. At the outset, the agency had five caseworkers, one casework supervisor, one director of casework, plus part-time psychiatric and psychological consultants. By the end of the first year of experimental service, as the number of girls grew and as service shifted from a casework to a group work emphasis, part-time treatment providers were employed. During the course of the project, ten caseworkers who had experience with groups were employed part-time; two part-time consultants in group work were also added. Except for some college women who volunteered to assist with the groups or to help individual girls keep appointments elsewhere, the treatment staff was thoroughly professional.

E. Treatment Population

The girls involved in the experiment (189 experimentals, 192 controls) were predominantly lower class and of minority status (approximately 58 percent black, 15 percent Puerto Rican, with the remainder white). Occupationally, parents of the experimental and control girls were largely semi-skilled, unskilled or domestic workers; with better than one-fifth of all parents being unemployed. Of the mother and fathers, sixty-five percent or better had not completed high school.[1]

F. Dimensions of Treatment

(1) Contact Time

Because it is unknown how long the experimental service was provided the experimental girls, an exact estimate of the number and frequency of service contacts for experimental girls is not possible. However, it is known that 129 experimentals were in the vocational school three years (these girls comprised the cohorts beginning in fall 1955, 1956, and 1957) and one cohort two years (the cohort beginning fall 1958). Almost all experimental girls were said to have received some treatment, and "half of these had 17 or more treatment contacts with social workers." Indeed, only 16 percent of the 189 girls in the experimental group had fewer than five such contacts, whereas 44 percent of them had more than 20 treatment contacts" (pp. 158-159). In the view of the research staff, this demonstrated that the experimental girls were "clearly well-exposed to the therapeutic program" (p. 159).[2] From what is presented, however, too much is in doubt to assess level of contact.

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[1] Occupational and educational status of parents not given for all experimentals and controls; information provided for 120 experimentals and 132 controls.

(2) Treatment Plan

The experimental program had, of course, the endorsement of the school administrators and staff; referrals were made in the name of the school to the YCS agency which was located within walking distance of the school. Service was provided during school hours, with girls being excused without penalty to go to the agency. When the agency moved during the course of the project, service continued to be provided under YCS auspice in a nearby YMCA. Parental permission was needed for the girls to be accepted into the service.

Initially, YCS planned to give casework services to the experimental girls. After about one year's experience, however, the treatment plan was changed to provide group work rather than individual case service.

The fifty girls comprising the first cohort of experimental subjects appeared apprehensive about being selected and about the personal nature of casework service. As a result, caseworkers did not feel that many of their clients became seriously involved in a treatment relationship. Starting with the second cohort of experimental girls and continuing with the following two cohorts, group work was given; a total of 139 girls, out of 147 referred, accepted the group service. An exact explanation of why the girls were selected in the first instance was never provided; group service, however, made it somewhat easier to rationalize treatment by de-emphasizing the individual problem orientation and stressing the "universality of problems and the provision of pleasurable experiences" (p. 97).

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[2] On the other hand, a case can be made that the experimental subjects were not well-exposed. For example, let it be generously assumed that each of the 129 experimentals was exposed over a three-year period and that each had 23 service contacts. Subtracting school vacation time of nine months from three years, there results an average of .93 contacts per month per subject receiving the maximum amount of service--scarcely an argument buttressing a claim that the experimental girls were well-exposed to the service.

The first five groups were organized in 1957-58 and were composed of thirteen members, a size considered somewhat large for traditional therapy groups but big enough to assure a resistant girl sufficient anonymity at the outset. These initial groups appear undifferentiated in membership, but no clear picture is given so that group composition and activity are unclear. These initial groups were reported to lack cohesiveness and to be inappropriate for some of the girls. Starting in spring 1958, therefore, five distinctive group configurations were instituted: (1) family life education groups which were essentially educational rather than "therapeutic"; (2) interview treatment groups for the more "intelligent and verbal girls" who used these groups to discuss their conflicts with parents and siblings and "to exhibit neurotic problems" (p. 107); (3) protective groups for "intelligent, verbal girls" who had to "cope with severe environmental problems" (p. 107); (4) activity groups for girls "at a low level of development" who resisted change who could enjoy supervised activities; and (5) observation groups which were short-term in nature (five to eight sessions) and used by the staff for assessing the last 80 experimental girls in order to refer each girl to one of the four kinds of groups discussed above. Each family life, interview treatment, protective, and activity group was limited to eight members.

Overall, the treatment plan appears to have been twice revised once service began. Casework as traditionally given by YCS was found inappropriate for the girls in the first cohort, prompting a move to group service. While groups appeared more promising, the first experience with undifferentiated groups was considered too unfocused in a treatment sense to be optimally useful; more highly differentiated groups resulted. Throughout, the staff rationalized the revisions in treatment procedures in terms of its theoretical orientation. Only the interview treatment groups approached the aims of "therapy" as the agency defined it but even activity groups were useful because they provided experiences that the girls "probably missed in earlier years" (p. 108); all groups attempted to be "ego building."

(3) Involvement

At the close of the project, the social workers were asked to rate the involvement in service by the experimental girls. Of the girls in the first cohort who received casework services, 47 were rated; almost half of these (47 percent) were considered "hardly or not at all" involved, while twelve more were seen as "some or a little" involved, leaving a remainder of 12 considered "very much or quite a bit" involved. Group service was, by worker estimation, better received. Of the 127 girls rated, 51 (40 percent) were considered "very much or quite a bit involved," 51 were rated "some or a little involved," while 25 were seen as "hardly or not at all involved" (pp. 149-150).

G. Findings

Major measures of outcome consisted of the performance records in school established by the experimental and control girls. At the close of the project, experimentals and controls were indistinguishable on the measure of final school status; 48 percent of both groups had graduated or were in normal grade. (This performance was below that for those girls who were not chosen for the experiment; 65 percent of them had progressed normally through the school system.) A slightly smaller percentage of experimentals (52 percent) than controls (56 percent) had been suspended or discharged from school during the life of the project, but this difference was not statistically significant. The percentage of failing grades for both groups was essentially similar; there was no appreciable difference between the two groups' records of attendance and no difference was discernible in grades given for conduct. Experimentals did appear a bit less truant than controls.

Out-of-school records, insofar as they were checked, showed that 13 experimentals (7 percent) and 9 controls (5 percent) were involved in court proceedings. Twenty-three (23) controls and 18 experimentals became pregnant out-of-wedlock, a difference so small as not to be significant.

More subjective measures of attitudes--possible changes in the girls' assessment of the future and the like--showed little difference between experimentals and controls.

Regarding the objective school and out-of-school measures, "no strong indications of effect are found and the conclusion must be stated in the negative when it is asked whether social work intervention with potential problem high school girls was in this instance effective" (p. 180). Regarding the self-reported indicators relating to attitudes, self-perception, view of the future, etc., these failed "to detect in any important respect an effect of the experimental treatment program" (p. 204).

H. Recommendations

The failure of this project gave rise to the following recommendations: (1) Whereas the project grew from a desire to reach girls before they were in serious difficulty, the experience with girls who had not yet clearly exhibited a problem showed that service tended to be unfocused and ambiguous; "treatment should be made specific to the expressly diagnosed problem" (p. 209); (2) work with lower class girls may more appropriately focus upon more concrete goals, such as helping a girl complete high school, rather than "achieve a psychological orientation exemplified in a middle-class style" (p. 215); and (3) it is appropriate for social agencies "to point change efforts toward conditions directly affecting situations determining these outcomes (that is, poor school performance, out-of-wedlock pregnancies, etc.) as well as toward changes in the clients themselves" (p. 216).

In summary, while not abandoning a commitment to an ego-psychological theoretical orientation, the YCS suggests a "multi-level" attack which would incorporate a social reform component.

The Chicago Youth Development Project, Chicago, Illinois

(Gold and Mattick, 1974; subsequent parenthetical references in this section, unless otherwise noted, are to this work.)

A. Background

In 1956, the Boys Clubs of America, one of the largest national youth-serving organizations, initiated an innovative youth program in Chicago designed to serve predominantly black youths residing largely in high-rise public housing on the city's west side. The usual Boys' Club program is building-centered, with boys seeking out the recreational and leisure-time activities to be found at the clubs. The innovative program sought to involve the more troublesome boys who did not partake of the traditional program; the new program would assertively seek out such boys by employing "extension workers" who would go into the streets and devise programs not so totally dependent upon one permanent facility. As it evolved, this new program had two service aspects: (1) street work with boys, often found in loosely organized groups, and (2) a community organization effort which aimed at having parents and other adults promote youth improvement endeavors and address such broad issues as community safety, integrating the tenants of public housing with persons residing in the larger community, etc.

The generally positive experience with this innovation gave rise to a desire to research the effectiveness of the dual-focused program. With grants from the Ford Foundation, the W. Clement and Jessie V. Stone Foundation and the U.S.

Department of Labor, a six-year action-research project was carried out. The project began in 1961 and ended in 1966. The first year (1961) was essentially given to recruiting and training both service and research staffs; the next four years (1962-65) saw the implementation of the program to be tested; and the last year (1966) was taken up largely with closing the project out, report-writing and the like. In all, the six-year project cost $1,410,000.

B. Theoretical Orientation

The project staff rejected the notion that at the heart of delinquency was the disturbed individual; rather, "high delinquency rates reflected social conditions" (p. 37). The staff also rejected the concept of closely-knit delinquent gangs as the peer-oriented milieu generating antisocial acts; economically depressed inner city boys simply are not capable of "stable, elaborate and tight organization. . ." (p. 37). The project staff did espouse a complicated theoretical melange which held that in varying degrees delinquency could be explained by interlocking factors, namely: (1) A sense of powerlessness pervades the inner city because of endemic poverty which makes the area vulnerable to exploitation and social normlessness; (2) the inner city is likely to have ethnic subcultures which deviate from and collide with the dominant culture; (3) the inner city is deliberately used and cynically manipulated by those in political power as the place where illegal behavior will be tolerated; (4) parents in the inner city are not able, because of poverty and chaotic social relationships, to acculturate their children to the dominant norms; (5) unacculturated children are likely to form youth-oriented counter-cultures which differ both from the parents' subculture and the wider, dominant culture; (6) inner city family life, consequently, is unstable and (7) youths look to peers, who band together in loosely organized groups with shifting memberships, for ways to fulfill social needs; (8) youth-oriented institutions, notably the schools, are alien to and antagonistic toward this delinquent-prone counter-culture; (9) and in any event, the youths gain a sense that they are stigmatized by race and poverty, that legitimate opportunities in terms of employment are not likely to open up to them whereas illegitimate opportunities (such as drug dealing) might, and that should neither legitimate nor illegitimate means present themselves, fighting, drug taking, and crime are ways to vent frustration (pp. 30-40).

C. Research Design

The staff selected two inner city neighborhoods as the targets for the experimental services: a predominantly black area, the Henry Horner area, situated two miles west of Chicago's central business district; and an area which Italians, Mexicans, blacks and Puerto Ricans shared, the Old Town area, wherein the Italians were the dominant group both in terms of numbers (one-third of the population) and as a political force. The two selected areas were in close proximity geographically. Because the interventive strategy called for providing community organizing with adults and street work with groups of boys in a manner which service workers considered appropriate, any person in the two target areas had to be viewed as a potential recipient of service. Consequently, control groups could not be drawn from the Henry Horner and Old Town areas. Using 1960 census data and the results of a block survey conducted by project staff, the researchers concluded that a satisfactory match could not be altogether made between the two target areas selected for service and two non-service areas. As a result, two kinds of control areas were utilized: (1) Two "natural-unified control areas"--that is, areas having definable boundaries and having some but not all of the salient characteristics of the service areas--were matched, one-to-one, with the service areas; and (2) "constructed-dispersed control areas"--that is, smaller areas, not having a distinctive demographic configuration--were used in order to correct for the deficiencies of the "natural-unified control areas." This technique permitted a reasonable match on such discrete variables as total population, percent of males ages 10 through 19, non-whites, average size of household, median family income, median school years completed and rate of employment (p. 57).[3]

The service phase of the study ran for four years, 1962-1965. Outcome was to be measured along five major dimensions:

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[3] The Henry Horner area appears to have been better matched than the Old Town area which, on such variables as percent of non-white population and percent of blacks to whites, was not as closely matched with its control areas.

(1) boys' standards of behavior; (2) school performance; (3) jobs; (4) constructive activities and (5) amount and types of delinquent behavior. The male population, ages 10 through 19, residing in the two target populations was designated as the group to be served directly by the extension (street) workers, and less directly by community resource coordinators. There was no preservice selection of precisely who would be served.

D. Treatment Providers

The project was conceived largely by sociologists and psychologists having university affiliations. "The Project Director hired most of the staff, and he preferred applicants whose training was in a social science such as sociology and psychology to those trained as teachers or social workers" (p. 271). Whether a worker needed an undergraduate or advanced college degree is not stated, although from the occasional references made to the age of the workers, it is assumed an undergraduate degree sufficed. Additionally, the workers had to have some "feel" for street life and the politics of institutions operating in inner city areas; being 'radical,' 'hip,' or 'far-out' were not seen as detrimental to the kind of work that needed to be done.

At any one time, there were approximately seven extension or street workers and four community resource coordinators in the Henry Horner area, two extension workers and one community resource coordinator in Old Town.

E. Treatment Population

The project was concerned with modifying the delinquent and antisocial behavior of young males residing in the two service areas, and it is this population that will be discussed here. It should not be forgotten, however, that a good number of adults received service from the community resource coordinators. Because behavioral change on the part of adults was not the prime target, no rigorous attempt was made to measure service impact upon the adult behavior; adults were interviewed from time to time to determine their acquaintance with the project and solicit their opinion on such things as their perception of the cause of delinquency.

A clear statement regarding the workloads of the extension workers over the course of the project is not presented. Various analyses seem to have been made of worker contacts in 1963, and these accounts give rise to considerable confusion. In one place, it is stated that 186 boys were "reported to be the current case load of the CYDP workers" (pp. 188-189). (This represents approximately 26 boys per extension worker.) However, in another part of the report, there is a count of the number of boys known to the workers in the service areas. In the last quarter of 1963, the number of boys aged 10 through 19 residing in the service areas numbered 3,926. Of these, the seven extension workers were said to recognize perhaps 2,000 by name. The workers estimated that they had established relationships with 554 boys whose behavior the workers said they could influence positively (p. 161).[4] In yet another part of the report, there is a count of boys served after the "workers had been in the field for over a year" (presumably sometime in 1963) (pp. 184-185). This shows the seven workers having contacts with 598 boys; with 177 of these boys the workers estimated they had "maximum" influence and with 207 boys the workers claimed "some influence."

The discrepancies in all of these counts are nowhere reconciled. Some tolerance should be exercised, however, for given the latitude with which workers could designate boys they perceive as needing service and the discretion the workers used in determining amounts of service given, the count of boys being served could vary from time to time. By the close of the project, it appeared that boys 14 to 19 years of age received most of the services given (p. 264).

F. Dimensions of Treatment

(1) Contact Time

Given the confusion over the actual numbers of boys who could be said to have been served, the accuracy of the estimated amount of time spent with boys served becomes problematic. Again with reference to the analysis made after the workers had been in the field over one year, the workers were asked to estimate time spent with the boys. (On this dimension, workers rated 608 boys, not the 598 boys with whom they reported having "contact.") The workers estimated that they had daily contact with 170 boys; 103 boys were seen less than once a day but more than once a week; 243 once a week; 49 more than once a month but less than once a week; 43 once a month or less (p. 186). Contacts were either in a group or individual context and each contact lasted two hours.

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[4] Beyond the numbers in the workers' caseloads or realm of "influence," the workers claimed to know a great number of other boys who were less well-served, or not served at all. This rather amorphous group broke down into three categories: (1) boys with whom the workers had some but, by worker estimation, not influential association (1,130 boys); (2) boys with whom the workers had sporadic contact (1,670 boys); and (3) boys whom the workers merely recognized (2,150). These figures are at best inflated estimates because no attempt was made to correct for duplicate counting; two workers, for example, could report knowing the same boy as a sporadic contact, so that this boy would have appeared twice in the final count.

If these estimates are close to being accurate, they represent an extraordinary high level of worker contact, clearly eclipsing the service efforts of most other projects. The estimates are, however, scarcely credible. Assuming a five-day work week and assuming the seven workers saw but once a week all boys they claimed to have seen at least once a week or more frequently, then each worker would have been in prolonged contact with approximately 15 different boys each day of the work week. Included in the report are lengthy excerpts from a worker's account of his weekly activity (pp. 131-160). These do not at all indicate such a frenetic service effort as that estimated for all workers. Perhaps this particular worker's effort was low, but if so, the other workers would have had to reach even more than 15 boys a day in order to maintain the average. In short, level of contact is in doubt.

(2) Treatment Plan

Organizationally, the project attempted to retain affiliations with the Boys' Club by having the extension workers and community resource coordinators physically situated in the Horner and the Old Town Clubs and to a certain extent, having them under the administrative authority of the directors of these two clubs. Concurrently, program direction fell to an associate director of extension work and an associate director of community organization, both of whom were part of the project and not ongoing Boys' Club staff, but who, like the workers themselves, were partially answerable to the central administration of the Chicago Boys' Clubs. This dual administrative arrangement was never fully integrated. Also, the uniqueness of the project, the kinds of boys it tried to attract, the difference in service approach, the latitude of the project workers--all set the project staff apart from the regular Boys' Club staff and service philosophy, introducing palpable strains in the relationships between the two staffs, but not so much so as to seriously compromise the provision of the experimental services.

This is the only experimental project which directed considerably more than counseling services to adults in the community in order to "create enduring organizations of neighborhood adults that would be educated about the problems which give rise to delinquency, which would become committed to helping youth, and which might, united, exert effective political pressure on agencies and institutions on behalf of youth" (p. 83). The four community resource coordinators, then, created a second tier of service which was to complement the direct face-to-face service given youths by the seven extension workers. The coordinators were also to muster concrete resources, such as locating jobs, as the calls for such resources were passed on to them by the extension workers.

Extension workers were to focus primarily upon and remain in close contact with the boys in the two communities. This effort developed a service configuration that included recreational activities (swimming, pool playing, eating out, etc.); brokering between boys and social institutions (working out school plans for expelled boys, devising probation and parole plans with the courts and state institutions); role modeling (how to behave when getting a job, dealing with the police, etc.); providing concrete goods and services (getting a church to provide temporary shelter for a recently paroled boy); and forms of counseling, peacemaking, and advice giving (settling "lovers' quarrels," family disputes, and conflicts between rival adolescent groups; giving information regarding sex, drugs, etc.). While the extension workers had the Boys' Clubs in the two areas available to them, they usually worked away from the Club buildings. Having station wagons available permitted the workers to be independent and assertive, cruising the streets, taking small groups of boys to various places for recreational purposes, and going with individual boys who had to get back in school or see parole officers.

Overall, the service component had a "shotgun" pattern. Workers were allowed to designate adults and youths who, in their estimation, could profit from the services; these recipients could change over time and the amounts of services directed to them could vary. This is in contrast with the more typical service strategy employed in most prevention experiments; that is, a specific group of experimental subjects is initially identified and service is largely focused upon this group. The shifting nature of the services given in the Chicago Youth Development Project makes it difficult to know who specifically could be said to have been reached.

(3) Involvement

In 1963 and 1965, the research staff conducted structured interviews with "representative samples" of boys in both the service and control areas. Not altogether clear is how many boys in the service area were interviewed, although in the 1965 interview, it appears

184 boys who had some contact with extension workers were seen (see Table 5:4, p. 182). "All in all, 69 percent mentioned their worker sometime during the 1965 interview." Forty-six (46) percent named the worker as helpful when one was in trouble with the police; 40 percent said extension workers had 'something to offer'; 25 percent placed the worker "among the three people whose opinion he (the boy) most valued" (p. 177).

The researchers did advance some evidence suggesting that a high level of contact with and a positive relationship between worker and boy produced no greater success than did less frequent and less positive relationships. "That is, boys who reported in several different ways that they were tight with (that is, closely related to) their workers, that they depended upon them often for advice, and that they were frequent recipients of service, were the ones most likely to be out of school, hanging on street corners, and in trouble with the police" (p. 189).[5] The suggestion that there can be too much service for certain boys is useful, but it should not obscure the fact that there must be some level of worker contact persisting over time which service providers consider sufficient to enhance the likelihood of altering a boy's behavior. From the report, we do not learn what this minimum level of service contact may be, and how many boys received service in such amounts as to place them at or above this level. That approximately one-third of the boys receiving some service did not mention their workers when interviewed, while more than half could not relate the true purposes of the program indicates that for a majority of the boys (60 percent?) reached, involvement was minimal.

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[5] These high users of service were depicted as "stake animals," who appear "rotten ripe with the music of reform"; these boys served as a screen for other boys who did not want to be as close to the worker but who enjoyed the resources the worker provided (p. 195). The "stake animals," despite appearances, never seemed to stay out of trouble.

G. Findings

To quote from the report: "CYDP obviously was not a spectacular success. It did not effect major reductions in delinquency rates among its immediate clientele nor in the target areas which it served. It did not dramatically transform the lives and styles of its young clients nor markedly alter the quality of life in some inner-city areas of Chicago" (p. 263). The project may have registered limited success among older boys in the 16-17 age bracket, particularly in the area of keeping these boys in school. Dropout rates improved for some of the experimentals when compared with boys in the control areas but "these contrasting shifts are of a size which might be merely chance. . ." (p. 205). The attempt to place boys seeking employment into work situations was not successful; ". . .the overall employment rates of out-of-school boys in the target areas did not improve" (p. 218). Regarding improved leisure-time activities for youths in the target areas, "the project did not significantly conventionalize the boys' groups with which it worked, or the boys' groups in the target areas" (p. 236). In summary, by any measure, the project could not be termed a success: "not only did the project spread itself too thinly--over clients, over neighborhoods, over different methods--it also worked against the grain of its own agency" (p. 347).

The community organization effort was not considered well integrated with direct work with boys. While the community resource coordinators were hard-working, their efforts often were not sufficiently related to the aim of delinquency prevention to get identified by residents of the community as being specifically youth serving.

H. Recommendations

The project staff put forward the following recommendations: (1) Future efforts should not focus on hard-core delinquents at large in the open community, but seek out those boys who are "next most involved" in delinquency, splitting the latter off from the former who are unlikely to reform but who do get others involved in delinquent acts; (2) future street workers are cautioned against assuming that close relationships with boys constitute an indicator of success, for those boys who appear highly involved may be those less likely to change; (3) more effort should be put into strengthening family life, for "a boy's flawed relationship with his father has more potential for socialization than a highly satisfying relationship with a fellow who works for an agency" (p. 339); (4) if employment is to be a goal, it will need broad-based institutional supports such as a public full employment policy and viable vocational training in the schools; (5) while the CYDP gravitated toward serving older children, it would have been better to focus on younger children before they became involved in the juvenile justice system and bore the stigma that system imparts; (6) overall program emphasis should be placed on opening up educational and vocational opportunities.

A project that started out influenced by sociological theories of delinquency and explicitly rejecting psychiatric theories regarding the personality, ends by couching its recommendations in the theoretical framework of Erik Erikson, the famous psychoanalyst: "In Erik Erikson's terms,. . .a program would focus on replacing the negative--delinquent--role identity with a more positive identity. . . .It seems to us now that CYDP would have been more successful if it had concentrated its resources on such a program" (p. 347).

Seattle Atlantic Street Center Experiment, Seattle, Washington

(Berleman, Seaberg, and Steinburn, 1972:323-346; subsequent parenthetical references in this section, unless otherwise noted, are to this work.)

A. Background

In the late 1950's the social work staff of a small settlement house situated in Seattle's central area decided to limit its service to delinquent and predelinquent adolescent boys. The experience of working with such youths led the staff to question the effectiveness of the services given. A research proposal was developed which, beginning in 1962, was funded by the National Institute of Mental Health; supplemental funding was provided by the United Good Neighbor Fund of Seattle and King County. The experimental project lasted six and a half years (1962 through June 1968). The project was divided into three phases: (1) a three-year pretest phase during which time selection, research and service procedures were refined and initially tested (1962-1964); (2) the actual test or service phase of two to three years (1965-1967); and (3) a postservice phase (through June 1968) when the evaluation of service effectiveness was completed.

During the pretest phase, a small experiment was carried out in order to assess the feasibility of the study as originally conceived. This involved identifying a small group of appropriate junior high school boys as experimentals and controls and providing the experimentals with service; 21 experimental boys, with a control group of like size, received

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service for a five-month period. This mini-experiment and its results are not discussed here (Berleman and Steinburn, 1967:413-423). Only the conducting and evaluation of the major test phase will be elaborated.

B. Theoretical Orientation

Prior to the onset of the experimental service, the staff, with outside consultation, spent much time attempting to determine which delinquency theories could be shown to have particular relevance to the boys to be served. Specifically, the staff identified five possible theoretical orientations that appeared pertinent in explaining the delinquent behavior of central area boys, namely: (1) a theory of social goal, opportunity and deviant behavior, or the assumption that delinquent behavior in low-income areas was the result of the frustration of achieving "success" through legitimate means; (2) a theory of community disorganization, or the assumption that delinquent behavior increases when community controls break down; (3) the theory of differential association, or the assumption that delinquency is learned through high association with norm-violating peers and adults; (4) a theory of family disorganization and parental discipline; and (5) a theory of negative self-concept. Using a set of hypotheses derived from these theoretical orientations, a questionnaire was administered to seventh grade boys in the two junior high schools in May 1964. Because the project had the full cooperation of the police and schools, it was possible to identify those boys who had police and school disciplinary records; it was assumed that boys with antisocial records would respond differently to the questionnaire than boys having slight or no records. The results of the questionnaire did not reveal such a distinction; boys with records perceived social opportunities, community controls, their family structure and themselves much as did boys with no records. The results of the questionnaire did lend some support to the theory of differential association; boys with police and school disciplinary records admitted to knowing and associating with other delinquent youths more than did boys with no records.

The finding that delinquency may be learned through differential association suggested that the experimental service should aim at weakening or severing certain delinquent ties. Such an interventive effort was largely ruled out by the research design. The project had spent two years fashioning an experiment that monitored a cohort of boys uniform in age, sex and school attainment; "to abandon . . . these procedures to construct an entirely new network that would have allowed for selecting, matching, and monitoring delinquent friendship groupings would have consumed . . . the time budgeted for the service phase of the study" (p. 327). As a result, the experimental service was not firmly rooted in some theory or theories; ". . . the deliverers of service would come to act as essentially autonomous agents performing their obligations toward the recipients of service on the basis of somewhat similar but ultimately private rationales of intervention" (p. 327).

C. Research Design

The research called for providing the experimental service to 54 delinquency-prone boys matched with an equivalent number of controls. It was assumed that attrition would over time erode the 108 boys needed to fill the minimal number of experimental and control subjects; a pool of 125 boys was deemed necessary to conduct the experiment. In the autumn and winter of 1964-1965, the project's research staff examined the school and police records for the entire population of 421 entering the seventh grade. Discrete school and police offenses were weighed for seriousness and when the offense was committed, a more recent offense receiving a higher weighting than a like past offense. One hundred five (105) boys generated offense scores of a magnitude which the research staff considered indicative of high-delinquency potential. The remaining 20 boys needed to fill the study design were obtained through nominations made by seventh grade teachers who rated all boys on a seven-point scale of "no problem" to "great problem."

The 125 selected boys were ranked according to seriousness, with boys having high police and high school offense scores heading the list while boys having only teacher nominations were placed in the least serious position at the bottom of the list. Once ranked, the boys were divided into four categories: (1) those with severe records; (2) those with moderate records; (3) those with minor records; (4)

those nominated by the teachers. Through randomization boys comprising each of these categories were divided into experimental and control subjects. Those designated experimental (54 boys) were again randomly divided among the three social workers who were to provide the service; each worker thus received the names of 18 boys. Whenever an experimental boy became an attrition in the early stages of the experiment, he was replaced by a boy drawn randomly from the same control-group stratum. Fifty-two (52) experimental subjects, not the hoped for 54, were subsequently engaged in the program.

D. Treatment Providers

Over the course of the experimental phase, four professional male social workers, holding the M.S.W. degree, were employed as service providers; no more than three service providers were employed at one time. In addition, two project co-directors oversaw the efforts of the service providers; both co-directors were experienced, professional social workers (M.S.W.'s) but neither provided service directly.

E. Treatment Population

The boys were predominantly from lower class and public welfare households, headed usually by women as single parents. Of the 52 experimental boys who received one year or more of service, 43 were black, 5 white, and 4 other. At the outset of service, these boys were from 12 to 14 years of age.

F. Dimensions of Treatment

(1) Contact Time

Fifty-two (52) boys were engaged in service of from one year to two years (9 boys) to a maximum of two full years (43 boys). During this span of service, the service providers had approximately 18,000 service contacts of all kinds, from face-to-face interviews to written letters, with the 52 experimental boys and their significant others: mothers, fathers, siblings, other relatives, peers and others such as school teachers. On the average, then, each case (that is, one boy and his significant others) had 342 contacts during the full two years of service; it was estimated that in each case these contacts amounted to approximately 313 hours of exposure to the service provider. The number of contacts ranged from a low of 103 to a high of 952; hours of exposure to the service providers from 67 to 552.

(2) Treatment Plan

Once engaged, the boys were formed into groups of nine members each, with each service provider responsible for two groups. Groups met weekly in sessions lasting from two to three hours and because the service providers had station wagons, the groups had freedom to move about. Each boy was seen as part of a "client-system"; that is, the boy was perceived as being influenced by significant others, family members, peers, school personnel and others, who, along with the boy, comprised a system, any part of which could become the target for service.

(The exception to this was in the instance when a significant other may have been a control boy; service could not be directed to a control. As it turned out, control boys were seldom discovered to be significantly involved with experimental boys.) The weekly group meetings usually served to identify those boys who were having social difficulties; service providers would then begin to identify and work with those significant others who played a part in any particular boy's difficulty. "The service evolved a distinctive crisis character except for the weekly group sessions with the boys. Thus, intensive spurts of service were consumed by specific cases from time to time and by some cases all the time" (p. 332). Most contact was with the boys and their immediate families (better than 14,000 of the approximately 18,000 service contacts were with the boys, their mothers, fathers and siblings) (p. 333).

The service was a mix of treating problems and providing recreation. During the summer, the groups were taken camping. Overall, "the social workers were assertive and nonpunitive; they served as the recipients' staunch advocates; they mustered resources, counseled individually and in groups, and served as group discussion and recreation leaders. They focused on the boy and the family, and quantitatively the boy and his family received considerable amounts of the workers' attention" (p. 344).

(3) Involvement

No specific assessment of the quality of the involvement between service providers and boys was made. However, the service effort, in a quantitative sense, was considerable (see Contact, above). At the close of service, the research staff surveyed boys and their parents to determine how they perceived the effects of service. Fifty-four (54) percent of the boys reported that they believed their acting-out in the community decreased over the service period, whereas only 2 percent considered their community behavior worse at the close of service when compared with their behavior at the beginning of service. Seventy-one (71) percent of the boys thought their school behavior had improved during the service period; 12 percent reported their school behavior had become worse. Ninety-four (94) percent of the boys endorsed the service overall, saying they would participate in such a program again. Apparently, the boys came to know the aims of service, a majority thought it had helped them, and almost all boys enjoyed the program.

G. Findings

When compared with the control boys, the experimental boys showed no improvement on any measure and if anything, were worse during certain time periods. For evaluation purposes, the two-year service phase and the one-and-a-half post service phase were divided up into seven six-month segments; comparisons were made every six months. On the measure of

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school misbehaviors, experimental boys performed significantly less well than their control counterparts during the first six months of the service phase and during the first six months of the post service phase. Community behavior as indicated by police records revealed that the experimentals performed considerably worse than the controls in the third post service phase. Because the experimental group started with somewhat "worse" school and community records than the controls, the researchers reevaluated the performance indicators (school and police records) in such a way as to offset this disadvantage to the experimental group. This reevaluation procedure uncovered only one significant difference: The controls continued to outperform experimentals in the post service period on the school measures. Six experiments and six controls were committed to state juvenile institutions for delinquents during the service and post service phases. "Overall, the results of these analyses clearly support the rejection of the hypothesis that the services would significantly reduce the acting-out behavior of the experimental boys" (p. 341).

H. Recommendations

The final report concludes: "Adding the Center's experience to the past failures issuing from delinquency-prevention experiments, the evidence does seem to suggest that to help inner-city youths develop less destructive life-styles may require radically different interventive strategies, more comprehensive and system focused, as well as focused upon the individual" (p. 344).

Youth Development Program, Columbus, Ohio ((a) Reckless and Dinitz, 1972; (b) Reckless and Dinitz, 1970; subsequent parenthetical references in this section, unless otherwise noted, are to these works.)

A. Background

A group of Ohio State University sociologists, among whom was the well-known criminologist, Walter C. Reckless (Reckless, 1967), had been working on a delinquency prediction technique since 1955. This technique utilized nominations by sixth grade public school teachers who classified all boys according to their delinquent potential and by responses of sixth grade boys and their mothers to interview schedules designed to reveal the boys' delinquency proneness through measures of the boys' self-concepts. This attempt to identify "good boys" and "bad boys" just prior to the years of high delinquency was viewed favorably by the Columbus Public Schools. The research team was invited by the schools to carry their study one step further by implementing a prevention program for boys rated to have a high delinquency potential. With the help of National Institute of Mental Health funding, a demonstration-evaluation project was initiated in the eight inner-city junior high schools of Columbus.

B. Theoretical Orientation

The researchers had evolved a theory of delinquency which centered on a boy's self-concept. A good or positive self-concept had the power to promote "self-direction, steering the individual away from bad associates, cheating, misappropriation of other persons' money, drug abuse, overuse of alcohol, and other violations of moral and legal norms" ((a)p. 24). Because most children in so-called high delinquency areas will not become involved with the juvenile justice system, the researchers reasoned that delinquency cannot be adequately accounted for by such overarching factors as disorganized neighborhoods or structural impediments frustrating the attainment of socially desirable goals (Reckless, Dinitz, and Murray, 1957:17-25). It came down to individuals, some of whom had an element of "internal containment" which insulated them from delinquent behavior (Reckless, 1967:467-468, 475-478). To instill this containment factor required "good role models that have the capacity to direct the person toward acceptable activities" ((a)p. 24).

C. Research Design

Teachers and principals in 44 inner city elementary schools were asked to evaluate all male sixth grade students as candidates for delinquency by using three categories: (1) unlikely, (2) possible, and (3) likely to become delinquent. These boys would be passing into eight junior high schools where the preventive program was situated. The project ran for three successive school years: 1963-64, 1964-65, 1965-66. Sixth grade teachers made prior ratings on all male students in each of these years, so that researchers could identify the vulnerable boys and assign them on a random basis either into the eight special experimental classes (one experimental class in each of the eight junior highs) or into normal non-

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experimental junior high classes. Four carefully selected male school teachers conducted the eight experimental classes, each teacher being responsible for classes in each of two junior highs. The experimental classes were given to boys during the seventh grade.

In summary, the research design called for three cohorts of seventh grade boys who were randomly assigned into experimental or regular classes; these cohorts followed one another, 1963-64, 1964-65, 1965-66. Each cohort was carefully monitored; school and police data were collected on each cohort at the outset of the experiment, during the year in which the experimental classes were given, and each year for three years following. The three-year period of monitoring for each cohort after exposure to the experimental classes was to assess how effective the experimental classes had been in deterring antisocial behavior in school and the community as the boys moved into the "high delinquency years." The final evaluation of the last cohort of boys could not be completed until late in 1969.

D. Treatment Providers

The four male teachers selected to conduct the experimental classes came from the regular teaching staff; they were nominated by the assistant superintendent of schools and passed on by the project director who personally interviewed and studied the credentials of each teacher. Once recruited, these four teachers were given initial training seminars by the project staff, and once the project was under way, the teachers met daily after school with the project director for approximately two hours. Additionally, the teachers met once a week for two hours with a consulting psychiatrist with whom classroom behavioral problems could be discussed.

E. Treatment Population

The three cohorts of seventh grade boys comprising the experimental and control groups altogether numbered 1094 individuals, or 632 experimentals and 462 controls. (The project also followed the careers of a sample of boys who were predicted not to be candidates for delinquency; the comparison groups numbered in the aggregate 632 boys.) Prior to onset of the experimental classes, the experimentals and controls were essentially alike on the following characteristics: Their mean age was slightly in excess of 13 years at the time they entered seventh grade, or somewhat higher than the norm because of poor prior school performance; 50.9 percent of the experimentals and 50.4 percent of the controls were whites, with blacks essentially comprising the remainder of each group; 54.8 percent of all experimentals and 56.1 percent of all controls were from intact families; and a socio-economic ranking technique revealed that experimentals and controls were from the same lower socio-economic stratum (unskilled, service, and other low prestige jobs characterized the occupational level of family heads). Similarly, on school measures, experimentals and controls started in equivalent positions in terms of I.Q. scores (experimentals, 91.6; controls, 90.3); grade level attainment in reading (experimentals 5.7 versus controls 5.5); and average grade attainment. On balance, the aggregates of the three cohorts showed a close matching of all experimentals with all controls.

F. Dimensions of Treatment

(1) Contact Time

The boys in each experimental cohort were assigned to an all-boy "self-contained class" lasting for three class periods (2 hours) each school day. Each class ran for one school year and was comprised of approximately 35 boys. (It was the desire to reach this class size that made it necessary to assign more boys to the experimental group; the eligible pool of vulnerable boys did not permit a 50-50 split.) Because experimental and control boys were reported to attend classes better than 90% of the time, it can be concluded that experimental boys had considerable exposure to the teachers presenting the interventive regimen (2 hours per day, 5 days a week for approximately nine months, less absences about 6% of the time). This exposure time can be further reduced by approximately 40 hours because of the decision not to introduce a radically different teaching program to the experimental boys during the first four weeks of school in order to allay any sense that the boys were being treated differently from other seventh graders ((a)Appendix A, pp. 167-170).[6]

(2) The Treatment Plan

The four teachers were to be "role models" for the experimental boys, which meant that in addition to teaching prescribed curriculum content (English, geography, and social studies), teachers would present supplementary materials ("the role-model supplement"). This supplement consisted of introducing the boys (a) to the subject of work (the need for it, kinds of employment, job finding, etc.); (b) to the place of education in society and the behavioral skills needed to manage successfully the school experience (why rules are necessary, organizing time, dropping out as an unattractive alternative); (c) to the place and function of common governmental services (heavy emphasis placed upon the role of the police, judicial system, probation and parole; less emphasis upon such things as health and welfare services); (d) to the need for interpersonal skills and how to get along with others (appearance, attitude, proper conduct in employment); and (e) to the place of the family functioning (reproduction and care of the young, family problems, etc.). The teachers presented the topics in order and in a methodical, academic manner;

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[6] Appendix A of the final report, containing the class lesson plan, indicates that the experimental class met for 96 sessions, or a total of 192 hours in a period from late September to early May.

all teachers followed an agreed-upon lesson plan. A particular topic could take from approximately two weeks to better than 20 weeks to present. In addition, the teachers agreed to focus upon two other areas of apparent need: (a) reading deficiencies, and (b) establishing an acceptable code for classroom discipline (respecting the rights of others).

(3) Involvement

The project appears to have been totally boy centered and classroom focused; parents and significant others of the experimental boys were not targets for special forms of interventive services. The researchers came to question the meaningfulness of the interaction between teachers and boys and of some of the lesson plans, but presented no evidence to support this assessment ((a)p. 158). A follow-up interview of experimental boys in the 1964-65 cohort, conducted two years after these boys had completed the experimental educational program (1967), revealed that the boys recalled the experience with enthusiastic approval and thought they had benefited from it. Unclear is how knowledgeable the boys were about the actual purposes of the program. At the outset, it was agreed that the boys would receive no explicit interpretation of the project's aim; assignment to the experimental program was masked by the teachers saying they had selected those boys they wanted but nothing more. This was to guard against "the possible adverse effects of negative labeling; the project staff did not want the boys to believe they were conducting special classes for the "bad boys."

G. Findings

Each cohort of experimental and control boys was cleared through police and school files once a year for four consecutive years. For all 632 experimental boys, 20 percent were known to the police prior to the program, 12 percent became known during the program, and 38 percent became known in the three years following exposure to the program, so that approximately 48 percent of all experimentals had a police record by the time they were about 16 years of age. The police records for the controls were almost identical; of the 462 control boys, 19 percent were known before, 11 percent during, 36 percent three years after the project, with 46 percent of all control boys eventually having records by roughly age 16. When the police offenses were stratified by degree of seriousness (serious, moderate, slight), the experimentals and controls continued to look alike (experimentals 28.8 percent serious offenses, 35.3 percent moderate, and 35.9 percent slight; versus controls 30.6 percent serious, 32.1 percent moderate, and 37.3 percent slight).

The school measures were no more promising. Although slightly fewer of the experimental boys dropped out of school (19.1 percent) than did controls (22.7 percent), this difference is so small that it could be attributed to chance alone. On such measures as truancy, attendance and mean gradepoint scores, experimentals and controls scarcely varied before, during, or after the project.

"In summary, the police and school data unfortunately failed to sustain the hope that the Youth Development Project would effectively prevent delinquency involvement and school dropout among inner city boys. Instead, every measure indicated little or no difference between the treated and untreated nominated predelinquents" ((a)p. 111).

H. Recommendations

The researchers regretted that sensitive measuring devices for assessing fine changes in behavior, attitude, self-concept and self-control were not available to them. Official school and police records were considered crude indicators at best, incapable of making sensitive discriminations. A more subtle assessment needed to be made of the possible impact of the program, but no such assessment could be made because of the measuring devices available. How to make effective adult role models and intensify and extend the efforts of such models were also given as needs in the field.

Opportunities for Youth Project, Seattle, Washington (Hackler, 1966:155-164; subsequent parenthetical references in this section, unless otherwise noted, are to this work.)

A. Background

This project grew directly out of a research proposal to the Ford Foundation submitted by a sociologist earning his doctorate at the University of Washington. It is one of the few projects which was neither the outgrowth of a preexisting service nor grafted onto an established youth-serving institution, such as a social agency or public schools. Starting in January, 1964, the project was situated in four low-cost public housing projects in which there were relatively high rates of official delinquency. From the final published report, it is impossible to tell precisely how long the project was in existence, although it appears to have lasted for at least one but not more than two years (1964-65).

B. Theoretical Orientation

OFY researchers postulated that delinquent behavior may be the ongoing response to significant others--parents, peers, teachers, employers--who project the expectation that these youths will be deviant. They rejected the more common assumption that deviant norms are deeply entrenched in the youths' egos which then necessitates the efforts of adult change agents who attempt to teach or instill more acceptable, normative behavior. "Deviant activities are seen as the result, not of internalized values, norms, or attitudes, but of self

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image that emerges in the context of social interaction" (p. 156).[7] If deviant boys were exposed to persons who expected them to conform, these boys would in time conform.

C. Research Design

The project staff identified three key variables that they thought would produce positive change in the experimental boys: (1) paid part-time employment; (2) success in mastering academic subjects through the programmed instruction of teaching machines; (3) getting adults in the broader community to view adolescents more favorably. Not all experimental boys in the four housing projects would be exposed to all of these key variables; the variables would be manipulated in such a way that some experimental boys were exposed to all, while others would be exposed to fewer of the interventive variables. Furthermore, the paid work experience would come in three different forms, so that this too would be graded.

In each of the four housing projects, four experimental groups comprised of ten boys each were to receive some variant of the interventive program. A 20-boy control group was also to be in each of the four housing projects. The four experimental program variables were as follows: (1) One group of experimental boys in each housing project was to receive paid part-time employment under "informal" supervision; that is, supervision was to be under an adult male who communicated to the boys under him that they were normal, capable, responsible, non-deviant and adequate; (2) one group of experimental boys in each project was to receive paid part-time employment under "formal" supervision; that is, supervision was to be under an adult who was more "rigid" and less communicative in his positive expectations for and views of the boys; (3) one group of experimental boys in each project was to be given individualized, part-time work in the surrounding community under minimal adult supervision; (4) experimental boys in the fourth group did not get paid part-time employment; however, in two housing projects these boys took part in "testing" teaching machines, whereas in two other housing projects, these boys were put on a "waiting list" which gave them "hope" of employment. The teaching machine testing program also extended to experimental boys who had part-time work in two of the housing projects.

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[7] Although the final report of OFY makes no mention of it, this theoretical position appears to have much in common with the well-known theory of delinquency and opportunity advanced by Cloward and Ohlin in 1960. Cloward and Ohlin maintained that youths living in deprived areas shared the American goal of seeking material gain but were denied legitimate opportunities by which to reach the goal. This frustration of legitimate means prompted some deprived youths to employ illegitimate means, if such means were available to them, to achieve the goal; organized crime being the example of an illegitimate way to achieve material success. Those low-income youths who had neither legitimate nor illegitimate means available resorted to "senseless" crime; these youths would be particularly attracted by legitimate opportunities if made available to them (see Cloward and Ohlin, 1960).

In other words, in two housing projects experimental boys in groups 1 ("informal" work supervision), 2 ("formal" work supervision), and 3 ("minimal" supervision) were to "test" teaching machines along with group 4 boys (teaching machine only) in these two housing projects. The other two housing projects had no teaching machine program for the experimental boys.

Finally, the initial research design called for an effort to be made by project staff in two of the housing projects (one project with and one without the teaching machine component) to develop a public relations campaign that would dispose adults in those two communities to view adolescents in a more favorable light.

In terms of design, this project was unquestionably the most elaborate. In all, the experimentals and controls broke down into 20 unique groups which was to have permitted a complex analysis of the many variations in order to determine the most potent mix of experimental variables.

Implementing this multi-faceted design proved difficult. The plan to change the attitudes of adults in two of the communities was abandoned. The "testing" of teaching machines was to have been done in such a way that the boys were to have the impression they were recommending whether the machines would be helpful in regular classroom situations. It was unlikely that this condition pertained; remediation probably became evident. The distinction between "formal" and "informal" supervision proved not to be meaningful, although the "informal" groups did have somewhat more latitude in job selection and the like. The size of the groups were difficult to standardize, and two of the planned-for 20 groups never materialized. Nonetheless, the project staff considered that "the main theme of the program was carried out. Boys were placed in positions where it would be difficult for them to fail. . .we hoped that these boys would begin to see themselves as capable and adequate" (p. 159).

D. Treatment Providers

The final report does not make clear who specifically the treatment providers were and their backgrounds. It is assumed the work supervisors were recruited from the maintenance staffs at the various housing projects, but this is not clearly stated. Professional teachers were employed to supervise the use of the teaching machines.

E. Treatment Population

All 13-to-15-year-old boys who lived in the housing projects were considered eligible for the program. A pre-service questionnaire was administered to most eligible boys in the project. The boys were not told that the project was to improve behavior; instead, the boys were informed that there was needed work to be done in the housing projects and that part-time paid work would become available for some of them. Those who took the questionnaire were then randomly assigned to the various work and control groups. Of the 403 boys comprising the eligible pool, 200 boys took part (57 percent were black, 36 percent Caucasian).

F. Dimensions of Treatment

(1) Contact Time

Amounts of time, including the precise duration of the overall project, in which the various groups of experimental boys were engaged are not given. It was reported that experimental boys "were involved in the program for only a few hours a week" (p. 159).

(2) Treatment Plan

The treatment plan was to have followed a complex research design; as discussed above, parts of this design were abandoned. Not provided was a detailed discussion of what actually transpired.

(3) Involvement

No assessment of the boys' involvement was provided.

G. Evaluation of Effectiveness

The final report gave three broad measures of outcome which were used to evaluate effectiveness: (1) police offenses, (2) school indicators (referral to school counselors and ratings by teachers), and (3) findings of a questionnaire administered to experimentals and controls which assessed a boy's self-image, sense of alienation, etc. The evaluators would have claimed success if the project served to enhance a boy's self-image and modified in a positive way his perception of how others saw him and if the boy became less socially alienated. Curiously, the evaluators did not expect the "work program to reduce the official delinquency rate," because the project ran "such a short time" (p. 159). The final report provided neither police data relating to delinquency, nor school data which usually are used in assessing behavior, such as attendance records, grades and citizenship. Referrals to school counselors were used only to show that "good" boys in the study (those with no referrals to counselors) showed somewhat better results than "bad" boys (those who had one or more referrals) on the questionnaire items.

The evaluation, then, relied almost totally upon the results of the questionnaire. From this it appeared that work of whatever kind--formal supervision, informal supervision, minimal supervision--did not produce any significant attitude change on the part of the experimentals when compared with controls from whom work was withheld. One possible favorable outcome involved work coupled with exposure to the teaching machines; this "may have had some favorable impact on the boys" (p. 164). The evaluation concludes: "This preliminary analysis of the Opportunities for Youth Project counsels caution to other programs using employment as a means of delinquency control. Perhaps a more intensive effort would have had a measurable impact" (p. 164).

It should be noted that the meager presentations of the treatment data (actual contact time with boys, boys' involvement, elaboration of what treatment actually involved) leaves many pertinent questions unanswered. Because the project did not by its own admission attempt to alter delinquency as officially defined, the conclusion that work as a potent treatment variable must be viewed cautiously in "delinquency control" appears ill-founded. Finally, however elaborate the research design, the actual implementation appears from the evidence available to have been seriously compromised and of short duration; the adequacy of effort to effect desired outcomes is in doubt.

Wincroft Youth Project, Manchester, England (Smith, Farrant and Marchant, 1972; subsequent parenthetical references in this section, unless otherwise noted, are to this work.)

A. Background

This project was sponsored by the Youth Development Trust, a youth-serving organization whose function was not fully elaborated, and jointly funded by the Department of Education and Science, the City of Manchester, and the Home Office (pp. 8-9).[8] In conceptualizing the project, its originators, a small group of professional social workers, were influenced by the service model of the detached street workers such as that pioneered by the New York City Youth Board, by the Cambridge-Somerville study with its incorporation of experimental research, and by the history of a teen canteen operated in London (1955-62) which successfully attracted many street youths. (One of the project's originators had worked at this canteen.) Not affiliated with any pre-existing program, the project had to seek both a suitable staff and location once funding and a managing committee were assembled. Also to be developed was a service program which at some point would be the subject for evaluative research. Events unfolded as follows. A planning phase (October 1963-March 1964) culminated in the opening of a teen canteen near the center of Manchester in a lower-working-class area (called

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[8] Funding sources are not altogether clear because of an ambigious presentation. For example: "The Home Office offered a grant of £9,340 over four years to 'group work in a High Delinquency area.'" Was this grant accepted?

"Wincroft," which is not the area's true name) having the seventh highest delinquency rate in the city. Between February 1964, when the canteen opened, and January 1966 when the canteen was purposely closed in order to inaugurate the project's experimental service and evaluative research phase, the project acquired three professional, full-time social workers and a number of adult volunteers. In this phase, staff and volunteers established contact with 184 youths in the area, and from this population drew most of the male youths who would be recipients of the experimental service. The addition of a fourth professional social worker to the staff and the selection of 54 boys for the experimental program occurred between October 1965 and March 1966. The interventive service began in April 1966 and was completed in September 1968. The services were given in and operated out of such facilities as leased store fronts, hired rooms, a gymnasium and a church hall. Heavy use was made of a minibus and the private cars of staff and volunteers. The services were deliberately shifted away from a building orientation; mobility was required as group activity utilized the community and region as places in which groups might go.

B. Theoretical Orientation

The staff of the Wincroft Project subscribed to no particular orientations; ". . .there was neither a theory, nor a theory of practice, to inform the workers in their effort" (p. 28). However, the professional staff was acquainted with psychoanalytic theory, behavioral theory, the theory of delinquency as a subculture, and the sociological theory of Albert Cohen who speculated that lower class boys who were to be judged by middle class standards they could not meet would then turn the system of values upside down, giving status to actions that were malicious, negativistic and nonutilitarian (pp. 28-31). An amalgam of two theoretical positions emerged; one saw the root of delinquency as a "failure in social relationships, usually with the mother or the father" (p. 29), and the other stressed the need of the immature, isolated boy prone to delinquent norms for sustained membership in small non-delinquent groups which would eventually be integrated into formal organizations (p. 30). Service staff were attentive to what they termed the boys' social "network." The attempt to build a sophisticated theoretical framework was, by staff admission, limited.

C. Research Design

The experimental treatment program would run two years (April 1966-September 1968) and involve 54 experimental boys between the ages of 14 and 17 when service was initiated (the average age was just under 15 years). A list of candidates was drawn up by (1) project service staff submitting the names of those boys frequenting the teen canteen who exhibited antisocial behavior; (2) the research staff compiling a list of those boys in the area who had previously been before the courts and convicted on two or more charges; and (3) teachers nominating "maladjusted" and "delinquency prone" boys by using the Bristol Social Adjustment Guides. Nominations by service and research staffs produced 71 names; teacher nominations reduced the list to the required 54 (18 with prior delinquency records; 34 deemed "maladjusted" and "delinquency prone" by scores on the Bristol Social Adjustment Guides, and 2 deemed delinquency prone but not maladjusted) (p. 22).[9]

A control group drawn from Wincroft was ruled out because the service staff wished to be free to involve other youths in the area besides the designated experimental boys if such others were seen as being meaningfully related to the experimentals. (The theoretical importance of a youth's social "network" necessitated this compromise of a randomly matched control group.) Additionally, the staff did not want to have to explain why some youths living in Wincroft received services while others, the probable control boys, could not receive service on any account (pp. 183-185). Behind this apparently lay the desire to mask the research endeavor from persons in the community. Consequently, the research staff matched the Wincroft area with another lower-working-class area of Manchester in which juvenile crime rates and such demographic characteristics as quality of housing, distance from city center, and population size were comparable. (Following the selection of the controls, the research staff also determined that school class size and attendance rates were

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[9] The Bristol Social Adjustment Guides consist of a number of statements about a pupil's classroom behavior; the teacher selects from multiple choices the one statement best characterizing a student's behavior in a given situation.

"identical" in Wincroft and the control area.) In terms of sex, age, official records of delinquency and scores generated on the Bristol Social Adjustment Guides, the controls were to be as much like the experimentals as possible. Seventy-four (74) controls were selected. On the three matching variables--age, two or more convictions and delinquency/maladjustment scores--the experimentals had "slightly fewer offenses, were slightly older (by three months on the average), were slightly less maladjusted and slightly less likely to become delinquent," but in no case was the difference statistically significant (pp. 192-195). As it turned out, experimentals and controls also proved alike in family size and number living in broken homes.

The research design called for a review of police records in 1968 and 1969. Experimentals and controls were to be interviewed in October 1968 to determine the boys' home, school, work, and leisure adjustment; and to elicit self-reported delinquencies (pp. 183-184).[10]

The research staff recognized two flaws in the evaluative design: "A completely satisfactory scientific design requires a control group and the assignment of clients at random to the experimental and control groups. This aim was not achieved. . ." (p. 182) and "the matching procedure used. . . is open to criticism in that it proved impossible with the available research resources to carry out individual

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[10] The follow-up interview utilized the Jesness Inventory (see Jesness, 1966).

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matching" (p. 193). The latter criticism meant that for each experimental boy there was not a control boy who on all relevant matching variables mirrored his counterpart. Evaluation would rest essentially on comparing the one group against the other.

D. Treatment Providers

Those giving the experimental services were four male professional social workers, one of whom was employed part-time, and a great number of volunteers, numbering from 20 to 40 at any one time. (From February 1964 to August 1968, 151 volunteers helped to provide service; not altogether clear is how many volunteers took part in the experimental phase starting in April 1966 and ending in September 1968.) Volunteers were of both sexes and ranged in age from 21 to 40, with the average age being around 26. The volunteers did not come with Wincroft, but came from middle class areas. Students comprised over 40 percent of volunteer total, with professionals (lecturers, teachers, social workers, clergy) being next most heavily represented (28 percent) (p. 35, 271).[11]

Because the volunteers lacked experience in working with difficult boys, the professional staff developed a screening procedure. Potential volunteers were screened by the project director. If acceptable to the director and with concurrence of the other professional social workers, the new volunteer was given a tour of Wincroft by one of the social workers. A youth group or particular boy was then selected for the volunteer and an initial meeting arranged with the social worker being present. In time, if all went well, the social worker would withdraw and the volunteer would become the primary deliverer of service under the social worker's supervision (p. 37).

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[11] The high proportion of students reflects the close tie the project enjoyed with the University of Manchester; evaluation of the project also benefited because of the relationship.

E. Treatment Population

The 54 experimental boys comprised a relatively homogeneous group; all were white (and non-Irish), were on the average 15 years, 3 months when service started, came from a lower-working-class community, and were likely to give up school for work. In one-third of the families, boys were without one of their natural parents. Cohesive street gangs did not exist in Wincroft, and the boys selected for treatment were seen as having "few, if any friends, and could not, because of the severity of their maladjustment, function adequately in a group situation" (p. 170).

F. Dimensions of Treatment

(1) Contact Time

During the term of the experiment, there was a total of 4,837 direct face-to-face contacts with the 54 treated boys, or an average of 90 contacts per recipient, about one contact every ten days (p. 158). The frequency of contacts ranged widely; 14 boys were seen 14 times or less (less than once every two months) while 11 were seen 134 times or more (about once every three days). At least 143 contacts were made with other agencies, employers, families and probation officers; the staff did not keep full records of such indirect contacts on behalf of the boys. Because much of the service was given to groups and group membership was not always comprised totally of experimental boys, the social workers and volunteers eventually served 600 Wincroft youths; "participants received just over 30 percent of workers' efforts in face-to-face situations" (p. 165).

(2) The Treatment Plan

The staff had no thoroughly structured treatment plan at the outset; ways of proceeding became highly idiosyncratic depending upon any particular boy's tolerance for service and needs as assessed by staff. The teen canteen was closed because it "posed problems of social control," and the staff, rather than attempt to impose "authoritarian methods" at the expense of alienating the most obstreperous boys, chose to abandon a building-centered program for one in which preservation of property was less an issue. The plan then was to reestablish contact with the boys whom the staff had designated as targets for treatment; the selected boys were not told specifically why they were of interest to the project staff. A pattern evolved in which the project staff, because of their resources--trips, access to sporting events, etc.--were accepted despite initial suspicions. Service in this phase was predominantly with groups; the workers did not form groups composed of experimental boys but instead identified the experimental boys and left it to them to suggest who might belong to the groups. A group could form in which there was only one experimental boy. The staff estimated that 60 to 85 percent of all work done was in the context of groups; for 20 experimental boys the service received was almost exclusively in groups (p. 170). In 14 cases, there was "little or no work"; in 16 cases, the initial contacts through groups permitted greater individualization and the giving of a mix of "group work" and "casework." The latter often involved helping boys find employment and living accommodations, representing the boy in court, and mediating with parents; "37 per cent of the total number of sessions during the evaluation period were casework sessions" (p. 171). Issues relating to education received little attention, possibly because in England lower class boys are permitted--indeed, often expected--to leave school early in order to seek employment and apprenticeships.

The 54 experimental boys and their associated natural groupings were initially divided into four separate caseloads with each load assigned to a social worker. The workers then received assistance in the form of volunteers; the volunteers could relieve the workers partially or totally in the provision of service to particular cases. Volunteers accounted for just under half the total effort from 1966 through 1968.

(3) Involvement

Fourteen (14) boys were minimally involved. "For the remaining 40 it can be said with some confidence that over a period of two and a half years they reached some common understanding with the 156 adults who at some time during that time tried to help them"(p. 99).

G. Findings

Evaluation of the project's effectiveness relied upon police records and post-service interviews with both experimental and control boys. Police records were reviewed in January 1966, December 1966, December 1967, July 1968, July 1969. Prior to service (January 1966), 46.3 percent of the experimentals and 48.6 percent of the controls had already acquired court convictions; the two groups were, on this measure, quite similar. Shortly before the close of the project (July 1968; the project ended service in September 1968), 54.4 percent of the control boys had been to court during the prior two years (January 1966 through July 1968), whereas 37 percent of the experimentals had court appearances in the same time span. "At the end of the project there was a significant difference in the appearance rate in court of the participant group in comparison with the control group" (p. 207). By July 1969, approximately a year after service terminated, this gap between the two groups narrowed; 62 percent of the controls and 50 percent of the experimentals had been to court; the difference was no longer statistically significant but was in the right direction. Additionally the number of convictions for those control boys appearing before the court was significantly greater than the number of convictions for the experimentals; "convicted controls were convicted more frequently" (p. 209). Not significant, but in the right direction, was the actual number of known offenses committed by experimental versus control boys; experimentals committed 63 offenses (an average of 3.2 for the experimental group) whereas the controls committed 179 offenses (an average of 4.4 for this group). However, when offenses were weighed for severity ('minor' offenses, and 'serious' offenses), 59 percent of the 63 offenses committed by the experimentals fell into the 'serious' category, while of the controls' 179 offenses, 53 percent were considered serious. The self-reported incidence of delinquent behavior revealed through post-service interviews "substantiated the findings from the police records concerning delinquency in the two groups. . ." (p. 215).

The personal interviews attempted to assess the boys' broader social adjustment. On measures of employment, home adjustment and social attitude, the interviews did not produce evidence that the two groups were significantly different, although the trends were in the right direction on all measures, with experimentals outperforming controls.

"On the basic question of whether or not the project was a success. . .the simple answer is cautiously positive. The results do not substantiate any more grandiose a claim" p. 239).

V. DISCUSSION

The outline used in analyzing the individual experiments will be followed in a more general discussion of the experiments considered together. Seen as a whole, the experiments generate questions and pose dilemmas which should be instructive for those wishing to do work in the delinquency prevention area.

A. Background

With the exception of the Cambridge-Somerville Study, the delinquency prevention experiments were a phenomenon of the 1950's and 1960's, an era when a post-war society was much taken with a concern for urban, teenage "gangs" and adolescent maladjustment generally. Being one facet of the increasing "crime problem," juvenile delinquency merited much official attention, as best testified to in the reports issued by the President's Commission on Law Enforcement and the Administration of Justice (President's Commission on Law Enforcement and Administration of Justice, 1967).

Against this backdrop, what is curious about the delinquency prevention experiments is their parochial character: that is, each delinquency prevention experiment grew entirely from local concern and interest. While various offices of the federal government eventually helped fund some of the experiments, the experiments were initiated by local persons and heavily dependent upon local sources.

A glance at the "Background" section of each experiment reviewed makes clear that a coordinated strategy to implement and evaluate delinquency prevention services has never taken place. Each experiment was one-time and idiosyncratic. So far as can be determined, no person prominently involved with one experiment ever went on to become involved with a second experiment.

Efficiency may not have been served by having each experiment conducted in isolation and often in ignorance of similar experiments. Cumulative experience may have been frustrated in never having some of these one-time experimenters get a second chance. On the other hand, the very insularity of each study lends a certain credibility to the cumulative findings of service ineffectiveness. By not being linked together, these independent studies achieve a distinctive and believable result: The convergence of largely independent evidence underscores how little is now known about preventing delinquency.

Finally, the cost of the experiments is unknown. Except for the Chicago Youth Development Project, cost figures were not provided in the final reports. Of course, some estimates could be made; because several projects had federal funding, their federal grant contracts could be traced and reviewed; but no attempt was made to conduct such a review. Even with this information, much would remain unknown about the expense of these experiments. Being the products of local concern, much of their support came from local sources to which access would not be difficult to gain.

B. Theoretical Orientations

With the single exception the experiments can be grouped under one of two broad theoretical orientations: an orientation which views delinquency as the result of unsuccessful or improper personality development; and an orientation which views delinquency as an understandable and not necessarily unhealthy response to environmental constraints which block lower class youth's access to legitimate socio-economic opportunities. Cambridge-Somerville, Youth Consultation Service, Maximum Benefits Project, the New York City Youth Board's Validation Study of the Glueck Predition Table, and perhaps the Youth Development Program stressed personality development. The others, with the exception of the Wincroft Youth Project, favored an environmental explanation. Those affiliated with the Wincroft Study appeared undecided and so did not clearly opt for one orientation over the other.

Under each heading, finer theoretical distinctions can be made. The work of the Gluecks was specifically cited in three of the studies: Cambridge-Somerville, Maximum Benefits, and the New York City Youth Board's validation study. In this view, the family, and particularly the factors of parental discipline and affection, are central in determining a child's adherence to norm or non-norm violative behavior. The Glueck's theoretical position is clearly compatible with the tenets of ego-psychology in which delinquency is but symptomatic of underlying neuroses, character disorders or psychoses; these disorders, too, are seen as rooted in child-rearing practices. Not surprising, then, that in two of the studies, the New York City Youth Board Study and the Maximum Benefits Project, the interventive service had a heavy psychiatric orientation as well as the orientation of the Gluecks. The Youth Consultation Service appeared not to be indebted to the Gluecks, but was in the ego-psychological tradition; this study appeared closely aligned with the previous two studies mentioned.

While the initiator of the Cambridge-Somerville Project expressed admiration for the earlier works of the Gluecks, Dr. Cabot in practice did not adopt a theoretically sophisticated position; he believed in old-fashioned, religious virtues as well as developmental theories of child rearing and did not insist that all treatment providers in his study share a uniform theoretical conviction. On the other hand, Cabot's naive view that a child's self-perception was the crucial factor resembled the self-concept theory later developed by the sociologist, Walter Reckless, who devised the Youth Development Program. Both Cabot and Reckless had a belief in the efficacy of adult, law-abiding role models to instruct delinquent-prone youth in proper behavior. Through such instruction a child's self-perception could be altered to insulate the child from antisocial behavior. What separates the Cambridge-Somerville and the Youth Development Program from the other studies grouped under the personality development heading is the faith the former studies placed in overt discourse and modeling behavior to change self-concept. The other studies, because of their ego-psychological orientation, appeared to accord such interventive directness with far less potency; the unconscious defense mechanisms would, in this orientation, protect the assaulted ego against such overt techniques.

Nonetheless, the theoretical orientations of these studies tend to make for certain similarities of approach. In general, treating younger children was preferable to treating older children. If the roots of deviance lay in a child's developmental history, then early intervention, or direct participation in that developmental history, was preferable to the theoretically more arduous task of remaking the well-formed personality construct of the older adolescent. The locus of intervention was largely the child or the child and the immediate family. Either as the professional therapist or parental surrogate, the treatment providers were engaged in healthy ego building.

The theoretical orientations of the remaining studies, while often noting the importance of the family and of child-rearing patterns, tend to stress the importance of socio-economic institutions and the hostile relationships that develop when lower class youths find accommodations with these institutions difficult or impossible. In this view, the antisocial behavior of socially disaffected youths is not emblematic of ego pathology but of understandable frustrations engendered by a denial of legitimate opportunities. Once clear access to education and employment is opened up, for example, the apparent deviance of lower class youth will moderate. These youths will, in short, be responsive to an altered opportunity structure and so conform.

Given this orientation, service providers should be mainly concerned with the interface between delinquent-prone youths and the key social institutions, particularly the public schools, which are crucial to ultimate social success and to the daily lives of most adolescents. Treatment providers are less therapists and parental surrogates and more advocates, facilitators, social brokers, and role models. Because ego development was less crucial, service providers had a rationale for working with older adolescents, and in the main, the population served in these projects were older than those served in the projects with an ego-psychological orientation.

As with projects concerned with child development and family life, differences can be noted among the projects having a socio-economic orientation. Only the Opportunity for Youth Project appeared to focus totally upon work and educational experiences to the exclusion of concern for the family. The Chicago Youth Development Project and the Midcity Project emphasized service to street groups and gangs, but did not ignore altogether the importance of the family. Both of these projects did have unevaluated services directed toward parents and other adults in the community. In these two instances, the services to adults appeared consistent in theory with services given the youths; that is, parents and community adults were to be organized into self-help groups aimed at community improvement, and lessening political powerlessness. The Midcity Project did make psychiatric consultation available, but saw "personality change" as a secondary effort. The Atlantic Street Center Project seemed to bridge both theoretical orientations, with greater weight given the socio-economic. While the individual families received the attention of workers, no attempt was made to organize these families; on the other hand, the Atlantic Street Center Project never sought psychiatric consultation or service.

The Wincroft Project was unique. Recognizing the range of theoretical orientations, it appeared to favor none. It resembled the American projects with a socio-economic emphasis; advocacy and facilitation were the distinguishing characteristics of the service providers. But unlike the American projects, access to education and employment did not seem as central in the English setting. For older adolescents, education was not required and not as socially prized, while employment appeared usually available. What emerged was a project given to the elaborate management of spare time activity of lower class youth for which no theoretical orientation quite fits.

Finally, none of the experiments constitutes a "test" of a theory. Validation of theoretical formulation in the social sciences is extraordinarily difficult and largely remains to be done. From the final reports of all projects, it appears evident that whatever the theoretical orientations, they served as general rather than specific guides in the actual giving of services. The theories themselves were so loosely construed that service providers could infer that whatever their efforts, they fell within the overall theoretical framework. Consequently, the negative results of the experiments do not constitute reasons for rejecting the theories; but conversely, had the projects been successful, the theories would not have been validated. From these experiments we can draw no reason to prefer one theoretical orientation over another.

C. Research Design

The use of the classic experimental design, or some close variation of that design, is the feature linking these projects together and giving credibility to their findings. But the very reliance upon this design can raise exceedingly difficult procedural and ethical issues.

(1) The Issue of Prediction

By definition, delinquency prevention services aim at moderating the rate at which antisocial acts get officially recorded. But which acts are recorded by which officials? It is not altogether clear which acts are worthy of the delinquent label. Most commonly considered delinquent are those youthful acts which gain the official attention of the police and courts, and which are not related to the problems of youthful dependency (the clear need for basic child support and sustenance) and perhaps incorrigibility (a parent's inability to exercise control over a child). School misbehavior is less clear-cut: Eating candy and chewing gum can lead to disciplinary action being taken by school officials, but by themselves scarcely appear to be delinquent acts. On the other hand, vandalism, serious fighting, and thefts at school are as serious as acts likely to be attended to by the police and courts, but may come to the attention of neither.

The dilemma in defining what is or is not a delinquent act poses a singular problem for delinquency prevention experiments and perhaps all services claiming to prevent delinquency. These experiments and services rest on the assumption that it is possible to predict with considerable accuracy those youths who in time will acquire delinquency records. How accurate have these predictions been? From the available evidence as provided by the experiments, it is difficult to know. School misbehavior tends to be recorded more often than the misbehavior recorded in police files, which is not surprising because the surveillance of youths in school is more intensive than of youths on the streets and because the schools tend to note infractions of school rules which can be quite minor, as the eating candy example points out. What is fairly clear, however, is that if prediction means designating those youths likely to come to the attention of the police and courts, then the experiments were not notably accurate. (The Youth Consultation Service Project is exempt; it did not claim to prevent community-based delinquency and so did not utilize police and court data.)

In the two largest experiments, Cambridge-Somerville and the Youth Development Program, better than 1,000 youths were evaluated. In a nine-year period (1938-46), slightly less than half (49 percent) of the Cambridge-Somerville experimental and control subjects became known to the police. Similarly, in the Youth Development Program, 48 percent of the experimentals and 46 percent of the controls acquired police records by age 16. Consequently, in the two largest experiments every accurate prediction was more than matched by a misprediction, and from what evidence there is, it appears that the other American experiments may have fared no better. Mispredictions pose no particular problems for conducting research cast in the mold of the classic experimental design, provided that these mispredictions are evenly distributed between experimental and control subjects as they appear to have been. But what the delinquency prevention experiments show is just how prevalent mispredictions are.

Serious ethical and perhaps legal problems begin to emerge if roughly half the youths likely to be engaged in a delinquency prevention service would never under any circumstances acquire police and court records. Is it prejudicial and potentially harmful to those youths who will not acquire such records to become targets for "prevention?" Furthermore, as we shall see in the discussion of service delivery, deception often was a distinctive feature in the giving of experimental services. Service deliverers were not candid in telling the experimental subjects precisely why they were the recipients of service.

Misprediction, when coupled with deception, constitutes a serious indictment of the delinquency prevention enterprise. There are no equivalents to the delinquency prevention experiments in the realm of crime prevention as that embraces an adult population. Adults without records but predicted to commit crimes have not been targeted for crime-preventing social services, probably because the presumption of guilt before proof of criminal behavior represents a dangerous erosion of constitutional guarantees. It should be remembered that the delinquency prevention experiments were conducted slightly prior to a time when the civil liberties of children were being seriously reconsidered by the courts and legal profession. But in light of current legal opinion, it can be wondered if such experimentation would now be vulnerable to legal action.

Finally, the ethical standards for conducting research have also undergone revision over the past decade. Governmental funding of research often requires that subjects understand the intent of the experiments in which they are to be engaged; deception, except under the most unusual circumstances, becomes increasingly difficult to justify. Given the extension of legal safeguards to children and the greater insistence upon full disclosure to experimental subjects, the viability of future delinquency prevention experiments may be in considerable doubt.

(2) The Issue of Alternate Research Procedures and Designs

Assuming that the current legal concern for the rights of children and the standards for conducting research free from deception make the further use of the classic experimental design problematic, does delinquency prevention experimentation have a future? Perhaps. Usually the experimental design designated particular *individuals* who were randomly divided into experimentals and controls. The tracking of unsuspecting individuals is what is most objectionable in a legal and ethical sense. There is an alternative to this precedent. The careful matching of neighborhoods, communities, and perhaps even towns and cities, and restricting an experimental service to one neighborhood, community, or town while withholding service to the matched counterpart--such a design may successfully counter objections. In this instance, prediction and evaluation need not rely solely upon individuals. Instead, one factor in matching would be the delinquency rate for a neighborhood being similar to the rate in another neighborhood. Evaluations of service would compare over time the rate of delinquency in one neighborhood versus the rate in a matched neighborhood. At the outset of the experiment, no specific individuals need be "predicted" to become delinquent and clandestinely followed over time. The service deliverers could provide services to whomever they wished within the target neighborhood, with the persons served never being identified for evaluative purposes. Simply the overall rates of delinquency generated by the two neighborhoods would be compared in order to assess service effectiveness.

If this modification of the classic experimental design appears reasonable, given current constraints, it also generates problems in its own right. In a research sense, the adequate matching of neighborhoods and communities is more difficult than matching individuals; suspicions about the comparability of anything as amorphous as neighborhoods could never be altogether laid to rest. The amount of service input would undoubtedly have to be increased considerably because the relatively few treatment providers needed when matched individuals are used would not be sufficient to offset a delinquency rate established by a neighborhood or community. That is, the design in which individuals are matched permits more economical service allocation than one in which neighborhoods or communities are matched and overall delinquency rates compared. Finally, it is likely that service providers would not cease making predictions about children. Traditional social services have been aimed at individuals and service providers would probably want their services directed toward children considered likely to acquire official delinquency records.

In this regard, the history of the two experiments which did modify the classic experimental design by matching neighborhoods rather than individuals is instructive. Both the Wincroft Youth Project and the Chicago Youth Development Project designated neighborhoods as the targets for services, but in each instance the service providers also focused upon sub-populations of particular children within those neighborhoods who were considered likely to generate delinquency records. To refrain from making predictions and to provide services indiscriminately within the target area would be asking much of treatment providers who desire success and therefore are understandably disposed toward bringing their services to the most "appropriate" children. Nonetheless, it would be possible to flood a neighborhood with a kind of youth service believed to prevent delinquency, and without regard to individuals, to compare overall delinquency rates. The Wincroft Project may have accomplished a kind of service flooding within a particular neighborhood and this flooding may have been as much a factor of that project's "success" as the services directed toward individual children. This will be elaborated further when treatment is discussed below.

(3) The Issue of Using the Term "Delinquency Prevention"

Of course the simplest way to avoid the problems that mispredictions inflict upon delinquency prevention projects and services would be to abandon the use of the term "delinquency prevention" altogether. However well established the term in psychological, sociological, and social work nomenclature, habit born of history is not sufficient reason to persist in what may be at bottom seriously prejudicial and libelous. If the delinquency prevention experiments teach us anything, they demonstrate how difficult it is to predict something as multifaceted as delinquency. The family may well be the ground from which youthful antisocial behavior springs, but it is the police and courts which detect the transgressions and affix the label which certify to the fact. To predict delinquency, one would not only have to assess family variables, as a good number of the experiments which favored the theoretical orientation of the Gluecks and ego-psychology attempted to do, but would have to assess as well the efficiency, beliefs, and tolerance levels of police and courts. This says nothing about the factors of chance and luck which have police at the scenes of some youthful episodes and not others. This cluster of factors no experiment fully took into account, nor indeed could be expected to. The many factors leading to citations in police and court records are too complex and unstable in combination to give hope that delinquency prediction will ever approach being a science.

The more recent research of those subscribing to social learning theory and behavior modification techniques often does not claim to prevent such a sequentially remote outcome as delinquency. Instead the goals of service are intimately linked with the treatment techniques; improved school behavior, family interaction, and peer relationships are typically the goals of behaviorally oriented studies. Everyone involved usually understands these are the goals; deception is minimal. Improvement in these areas may or may not be significantly related to what is eventually found in police and court records; enhanced school, family or peer relationships have to be seen as good in themselves, perhaps a lesser good, but nonetheless desirable. Researchers and treatment persons do not have to be behaviorists in order to have more modest goals. In those instances when individuals are clearly targeted for an experimental service, the behaviorists are pointing the way to a more prudent and circumspect research protocol that may have greater acceptability in an age when a child's rights are taken more seriously. The Youth Consultation Service is the one non-behavioral experiment which had the more modest goal of improving school performance without particular regard for officially defined delinquency.

D. Treatment Providers

In the main, the direct treatment providers in the ten experiments were college-educated with trained social workers being most heavily represented. Three projects appear to have used social workers exclusively (the Midcity, Youth Consultation Service and Atlantic Street Center Projects). Four projects employed a mix of treatment providers: New York City Youth Board Validation Study, Cambridge-Somerville, Maximum Benefits Project, and the Wincroft Youth Project. In these four experiments, trained social workers figured prominently in the giving of services, although psychologists and psychiatrists are in three of the studies cited as treatment providers. The Wincroft Project is unique in that a large number of middle class volunteers assisted four social workers in the giving of service. Cambridge-Somerville's treatment staff included a nurse and several unspecified "youth workers." The Youth Development Project used public school teachers as deliverers of service. The Chicago Youth Development Project deliberately avoided hiring social workers and teachers, preferring instead what is inferred to be persons with bachelor degrees in sociology. This study is not explicit on the backgrounds of its treatment providers, nor is the Opportunities for Youth Project which seems to have used some schoolteachers as well as "foremen" in public housing projects as treatment providers.

While it is not possible to render an actual count of the number of direct treatment providers, men predominated although women were clearly involved in four of the projects (Midcity, Youth Consultation, Cambridge-Somerville, and Maximum Benefits). Race and ethnicity of the treatment providers are not possible to determine.

Because no particular treatment orthodoxy can be said to exist within any particular academic discipline, it is unsafe to assume that all social workers, for example, were similar in their work in all experiments when employed as treatment providers. Those who conceptualized the experiments tended to have academic disciplines similar to those of the treatment providers. Only Midcity Project, conceived by an anthropologist, and the Cambridge-Somerville Project, conceived by a medical doctor, represented academic backgrounds not found in the treatment providers of those projects.

What the ultimate careers of the treatment providers may have been is unknown. If they should have mirrored the careers of those who devised and oversaw the experiments, the treatment providers probably did not continue on in the delinquency prevention field. It appears that however much delinquency prevention may be an ongoing interest in academic and some official settings, at the level of implementation it is a disjointed and sporadic enterprise.

E. Treatment Population

An accurate count of the total number of children involved in the ten experiments is not possible. Several experiments, notably the Chicago Youth Development and Midcity Projects, provided inconsistent counts, whereas others, such as the Maximum Benefits Project, were not specific enough to permit a count of the actual numbers involved. Nonetheless, what can be constructed from the admittedly limited data is of interest.

Three projects, New York City Youth Board Validation Study, Cambridge-Somerville, and Maximum Benefits Project, selected children aged twelve and under for study; five projects, Youth Development, Opportunities for Youth, Youth Consultation Services, Atlantic Street Center, and Wincroft selected children thirteen through fifteen; and two projects, Chicago Youth Development and Midcity selected children from early adolescence through late adolescence, or children and young adults ranging from thirteen to twenty-one. As indicated under a discussion of theoretical orientations (see above), the projects favoring a delinquency theory which had personality development as the crucial factor were those opting for a younger population (from age five through approximately thirteen). Projects emphasizing socio-economic factors in their theoretical orientations selected an older population (thirteen to twenty-one).

Only three projects engaged females, Maximum Benefits, Midcity and Youth Consultation Service, with the latter being the sole project in which females comprised the total study population. The experiments were overwhelmingly oriented toward males; delinquency has long been a social phenomenon synonymous with young males. Also evident is the fact that serious delinquency is perceived as stubbornly rooted in the lower class. All of the experiments reported their services were aimed at predominantly lower class youths, meaning children coming from the homes of blue-collar workers and public welfare recipients.

The factors of sex and socio-economic class are expected; what is more surprising is the extent to which delinquency prevention experiments identified non-whites as needing preventive services. Counting both experimentals and controls, the total number of youths involved in the American experiments exceeds 3,000.[12] (Wincroft, the English experiment, is excluded because of the different societal and cultural context in which it took place.) Of the total, approximately 41 percent is black, 3 percent Puerto Rican, and 55 percent white and other.

The Cambridge-Somerville Study represents a special case, being the one experiment conducted in the 1930's and 40's. Whereas 27 experimental subjects in that study are identified as being black, two-thirds of the youths are identified as coming from homes in which the parents are foreign born. In short, this study engaged an atypical population, reflective of a different era in American social history. Should the population of the more recent American experiments be studied (that is, excluding Cambridge-Somerville), the total black population exceeds 51 percent, Puerto Ricans are 4 percent, while whites and others fall to 44 percent. This is a conservative estimate because the many youths served in the Chicago Youth Development Project are not added in; their number is in doubt but they are known to be prodominatly black. This is inferred from the assignment of service providers in the Chicago project; five of the service providers were assigned to work in a black community, while only two service providers were detailed to a white community.

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[12] This rough count is derived as follows: Cambridge-Somerville, N=650; New York City Youth Board, Validation Study, N=58; Maximum Benefits, N=179; Youth Development Project, N=1094; Opportunities for Youth, N=200; Youth Consultation Service, N=381; Midcity, N=327; Chicago Youth Development, N=unknown; Seattle Atlantic Street Center, N=102; Total N=2991+.

The experiments, then, focused predominantly upon males between the ages of five and twenty-one, with the model experimental subjects probably being in the twelve-to-fifteen-year range, who came from the lower socio-economic class and who were non-white, if the Cambridge-Somerville subjects are disregarded. In one sense, the American experiments were similar in terms of selected youths. Perhaps the best way to link the early Cambridge-Somerville experiment with the later experiments is to view the youths selected as representatives of recent migrants to urban areas, the foreign born predominating before the Second World War, the blacks and Puerto Ricans after. In this context, juvenile delinquency appears as but one facet of a social assimilation process. The treatment providers, whatever their theoretical orientations, can be perceived as guides to a new society and culture, attempting to instruct urban immigrants in the acceptable modes of child rearing and of adapting to new instructions, such as the city school and employment situations.

Considering that none of the experiments was intimately linked to any other, it is surprising how consistently the American experimenters designated similar populations as targets for service. Here again the Wincroft Project was different. American service providers seemed to have worked with children quite different from themselves, while the English experimenters selected children who were of the lower class but otherwise much like themselves racially and ethnically. It can be wondered if in the American popular mind serious delinquency is in fact an attribute of youths who are distinctively "different."

F. Dimensions of Treatment

(1) Contact Time

The central independent variable in each of these experiments was the provision of a treatment regimen which was to retard or prevent delinquent behavior. Of primary importance, then, is an understanding of the nature of the contact treatment providers and subjects were able to establish. Contact can take two forms:

One is the number of times treatment providers had direct contact with the experimental subjects and the second is the number of times treatment providers had contact with significant others on behalf of the subjects. Contacts with or for experimental subjects consume time, so that the amount of time taken up by these contacts becomes the second vital dimension of the treatment variable. These are only the quantitative aspects of treatment; the qualitative aspects are much more difficult to define and assess but will be discussed below under Treatment Plan.

Because delinquent youths are not known to be sudden converts to normative behavior, it was implicitly assumed in these experiments that treatment must extend over a considerable period of time. No delinquency prevention experiment took less than nine months to complete, whereas the Cambridge-Somerville Project ran close to ten years. Exposure of experimental subjects and of their significant others--family, peers, school officials, etc.--to the variable of treatment becomes the most significant element in these experiments, for if treatment was not given or given in so few contacts and small amounts, then doubt is raised as to whether a meaningful test had been conducted.

Granting the importance of exposure to treatment providers, the most extraordinary fact which emerges from a study of the experiments is that this altogether crucial factor is so poorly documented. Furthermore, where some documentation is available, it is surprising how insignificant was the degree of exposure in many instances, so insignificant that the efficacy of a number of the experiments can be put into question. On the other hand, two experiments did provide a preventive service in considerable amounts and neither achieved a successful outcome. This is to caution that while the provision of treatment in substantial amounts may be necessary, it is not by itself sufficient to insure a desired outcome.

In the New York City Youth Board Validation Study and the Opportunities for Youth Project, the discussion of exposure to service was in such vague terms that it is impossible to know how many of the subjects received services in what amounts. The treatment providers in the Chicago Youth Development Project were apparently not required to keep systematic records, while the researchers' periodic attempts to estimate the street workers' contacts with or on behalf of youthful subjects yielded contradictory and highly questionable results. Similarly, an accurate count of the number of experimental subjects participating in the Midcity Project was not given. Midcity claimed that seven street gangs comprised of 205 individuals were seen by treatment providers 3.5 times per week over a period ranging from ten to thirty months. Because contact was explained in terms of service to street gangs and because the diverse individuals in these gangs may or may not have been present when the treatment provider appeared, it cannot be inferred that 205 individuals were seen 3.5 times per week. Street gangs are notoriously unstable in their membership (Richards, 1960; Yablonsky, 1959: 108-117), so that the number of contacts and amount of exposure is in considerable doubt in the Midcity experiment.

In three studies, treatment data are incomplete but do permit some inferences to be made. The Cambridge-Somerville Study did not attempt to quantify all treatment in terms of numbers of contact, direct and indirect, and the amounts of time these contacts consumed. But the Cambridge-Somerville treatment providers did keep a record of treatment contacts during the year 1940. Some of these contacts were with parents, school officials and others, but from available data, it cannot be determined precisely when the treatment providers were working directly with the experimental subjects and when indirectly. The average number of contacts per subject was 27.3, or approximately 2.3 per month; the time dimension was not reported. In the Maximum Benefits Project, 111 subjects and their parents were reported to have been served an average of eleven months. During this time, the average number of contacts per subject was 4.5 and per parent 10.9, or less than one interview per subject every two months and one interview per parent every month. Altogether, these contacts average 1.4 contacts per month; the time dimension was not reported. The Youth Consultation Service exposed 189 subjects to treatment; 129 subjects were exposed for a three-year period and 60 for a two-year period. Sixteen percent of the subjects were reported to have had fewer than five contacts, while 44 percent had more than twenty. With no clear statement which relates number of contacts to length of treatment, let it be generously assumed that each of the 129 subjects exposed for three years had 25 contacts. Subtracting school vacation time (six months from the three years), there results an average of .93 contacts per month per subject receiving the maximum amount of treatment. Again, the time dimension is not illuminated (Berleman and Steinburn, 1969:471-478).

Should the estimates of the treatment variable in these three instances be roughly correct, the contact with the experimental subjects was most modest. While no absolute demarcation exists which separates an inadequate from an adequate level of contact, nonetheless it can be wondered if from slightly less than one to slightly more than two treatment contacts per month with or on behalf of experimental subjects constitutes a level of treatment likely to impact delinquent behavior.

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The two remaining American experiments, the Youth Development and Seattle Atlantic Street Center Projects, appear to have achieved in a quantitative sense higher levels of treatment. In the Youth Development Project, experimental subjects are reported to have had 96 two-hour classroom sessions. The treatment classes met five days a week and ran through one school year, September to early May. Taking into account a reported school absence rate of six percent, it is estimated that each experimental subject was exposed to a treatment provider 90 times (about 181 hours) in a nine-month period. Apparently, the Seattle Atlantic Street Center Project was the only experiment which attempted to keep an accurate count of contacts and amounts of time for the entire period subjects were exposed to treatment providers. In the two-year experimental period, each subject and his significant others averaged 342 contacts, direct and indirect, which consumed approximately 313 hours of time. In 41 percent of the 342 contacts the treatment provider and subject interfaced (141 contacts per subject), with these direct contacts consuming approximately 84 percent of treatment time (263 hours per subject). In crude terms, this amounted to a treatment provider being in direct contact with a subject between five and six times a month with each contact lasting approximately an hour and three quarters. Indirect contacts were about eight per month per subject, which together lasted a brief two hours.

Treatment in the English experiment, the Wincroft Project, was not so intensive as in the Youth Development and Seattle Atlantic Street Center experiments, but appreciably more intensive than in the Cambridge-Somerville, Maximum Benefits, and Youth Consultation Projects. On the average an experimental subject was contacted by a treatment provider once in every ten days, for a total of approximately 90 contacts in all. Indirect contacts were not scrupulously monitored, so that no accurate picture emerges of this dimension. Similarly, the time factor was not reported. A unique aspect of contact was the extensive attention given not only to the experimental subjects but to other youths in the Wincroft target area as well. Eventually over 600 youths were contacted, so that the face-to-face contacts with experimental subjects amounted to only 30 percent of the total number of contacts made.

It is peculiar that these ten projects which selected the most rigorous research means to evaluate the effectiveness of their services were in the main so slipshod in monitoring and describing those very services. The final reports of four of the experiments either do not discuss their treatment services in a quantitative sense or describe them in such an incomplete and contradictory fashion that inferences cannot be drawn. Mitigating somewhat this failure to depict the most critical variable in these experiments is the fact that in three instances—Opportunities for Youth, Chicago Youth Development, and Midcity Projects—treatment took place in the streets and open community and was administered largely to groups and gangs. In such a context, treatment services become extraordinarily difficult to monitor with any accuracy; treatment providers make no attempt to keep a minute-by-minute account of their efforts but typically record their interventions at some time after the fact.

More perplexing are those instances where treatment was given in more circumscribed settings and often to fewer persons. The New York City Youth Board Validation Study, the Maximum Benefits, Youth Consultation, and Cambridge-Somerville Projects frequently gave services either to fewer persons at any one time or in settings, such as clinics, schools, and homes, which were well defined. Why the critical variable of treatment here was not better reported is subject for conjecture. Perhaps the treatment providers resisted keeping such information; Kandel and Williams (1964:109), in a study of treatment providers engaged in psychiatric research studies, concluded that treatment providers were not used to the systematic rigor required by research and "ignore directions calling for uniform procedures. . ." Possibly the information was kept but the writers of the final reports either did not see fit to include treatment data or could not readily use the data as recorded. The Cambridge-Somerville treatment providers, for example, generated 22,000 pages of single-spaced, typewritten service records kept in prose form; such an undifferentiated mass defies concise, summative statements regarding critical facets of the treatment enterprise. Only the Seattle Atlantic Street Center treatment providers consciously set about quantifying treatment data at the onset of the experiment, and by having treatment providers consistently record types of contacts and amounts of time on forms devised for computer processing, the dimensions of treatment came forth with some clarity. Clearly, the general failure to report the most mundane facts about the treatment services given constitutes one of the gravest shortcomings of these experiments.

(2) Treatment Plans

Most of the experiments provided theoretical rationales elucidating delinquent behavior which served as backdrops to the interventive treatment modes. In execution, were these treatment modes consistent with theoretical notions? A related question is: Just how much did the experimental subjects understand of these theoretical positions and of the consequent purposes of the treatment given them? The latter question will be discussed first:

(a) The Issue of Deception

One seldom-mentioned aspect of treatment procedures was the concealment from experimental subjects of the actual intent of those procedures. Where the issue of deception is mentioned in the reports of the projects, it is apparent that uniformly the experimental subjects were not told initially why they had been selected for special attention. The Cambridge-Somerville Project considered it necessary to select a large group of "average," non-delinquent boys in order to mask the true intent of the experiment from the boys who were predicted to become delinquent. The Youth Development Project deliberately had the treatment providers dissemble and initially provided the same instructional materials in the experimental classrooms as was provided in the regular classrooms so that the experimental subjects would not suspect that they were singled out for a special program. The Wincroft treatment providers were pointedly evasive throughout. Some experiments, such as the Youth Consultation Service and Opportunities for Youth, cloaked their specific concern about the deviant character of the study populations by couching treatment services in overarching abstractions, such as a concern for adjustment problems common to all adolescents or a desire to provide employment opportunities and the like.

No experiment reported that at the outset a candid explanation of the true purpose of the experiment was provided the selected youths and their families, or indeed, that they were the subjects for experimentation of any sort. What appeared to be the hope of the experimenters was that once the youthful subjects were engaged and their delinquent propensities became expressed in some form, then the treatment providers would respond in a helpful way. The subjects may have inferred from this response what the treatment provider was there to do, but seldom was this made explicit in the early stages of an experiment. An awkwardness pervaded the giving of services in many of these projects as if the mention of the experimenters' intent would make potential subjects refuse the offer of treatment services or work toward undesirable service outcomes. Despite the deliberate ambiguity surrounding their true nature, the experimental services were nonetheless surprisingly successful in initially engaging subjects. If few explanations were given, few penetrating questions appear to have been asked.

As mentioned previously (see Research Design above), evolving standards for conducting experimental research now make deception extremely difficult to justify, and if federal funding is involved, to employ. It is becoming agreed that human subjects have an ethical and perhaps legal right to know that they are objects of experimental research and to be told what the purpose of the research is. Whether the high degree of deception so evident in these ten experiments could continue to be practiced is doubtful. An intriguing question goes without an answer: Would youths predicted to become delinquent and the youths' parents knowingly agree to become subjects in a delinquency prevention experiment?

(b) Treatment Procedure

The obvious reason for using deception was the experimenters' fear that youths and families, which the professional literature had often described as "hard-to-reach," would not accept treatment services if put in such a negative context as delinquency prevention. The fragile quality in the establishment of ongoing relationships with the prospective experimental subject was further emphasized by the experimenters having few, if any, coercive means to insure that the youths would participate. Only the Youth Development Project using public school teachers as treatment providers and the school classrooms as the place of treatment and the New York City Youth Board Study using a child guidance clinic located in the school which the experimentals attended, could be said to have some implicit authority that encouraged good attendance at the treatment sessions. All other experiments had to trust that their services, being conducted in the open community, were so intrinsically attractive and useful that the uncoerced subjects would willingly tolerate the ministrations of the treatment providers. This appeared to be a "given" in all delinquency prevention experiments but two.

Set against this reality were the theoretical treatment rationales which supposedly guided the treatment services (see Theoretical Operations above). A palpable tension becomes evident in these experiments where the reality factor, that is, the experimenters' fear of losing youthful, uncoerced subjects, came into conflict with treatment procedures as dictated by theory. Most experiments appeared to accede to the reality factor by willingly modifying "ideal" treatment plans and procedures when it was feared the experimental subjects might withdraw. Or perhaps more accurately, most experiments couched their theoretical rationales in such encompassing terms, that whatever the behavior of the treatment providers, it could be rationalized theoretically.

Again, only the Youth Development Project had a thoroughly spelled-out treatment regimen in the form of a preconceived instructional program administered by carefully selected schoolteachers; the regimen was administered as planned, so underwent no major modification. In contrast, the Youth Consultation Service revised its overall treatment plan at least twice after first contact was made. The initial plan called for individualized casework services in the ego-psychological mode, but within a year this plan was dropped when subjects resisted a service having an individual problem orientation. Treatment was then given to groups of subjects; groups had the advantage of making problems appear more "universal," but groups comprised of undifferential subjects became unfocused. Undifferentiated groups were in turn abandoned in favor of specialized groups. None of these changes violated the underlying theoretical rationale; it could be said that all groups assisted in "ego building," while only "interview treatment groups," in which "neurotic problems" and parent-child-sibling relationships were discussed, approached true "therapy."

It is interesting that while therapy consistent with this project's theoretical stance occurred with only a minority of the subjects, the experiment was not terminated early. The Youth Consultation illustration is not atypical. Once started, the experiments were seen through no matter what the treatment revisions. Significant changes in treatment plans were evident in the Cambridge-Somerville, Opportunities for Youth, Wincroft, and Chicago Youth Development Projects. To a large extent, then, the treatment services that evolved in these studies were also experimental in the sense that they were not finely preconceived and rigidly adhered to. More important than any commitment to a narrow theory of treatment was the desire to keep the subjects engaged.

Because a good number of subjects were engaged, it is unfortunate that so little is known about how they were kept interested. If the more commonplace facts about the treatment services were not adequately described (see Contact Time above), the very sophistication of the experimenters and their understanding of extant behavioral and social theory may have stood in the way of a more objective accounting of what did in fact transpire as the experiments unfolded. What vignettes of treatment we have--and in this regard the Cambridge-Somerville, Chicago Youth Development, and Wincroft Projects, all reported in book length, are the most generous--suggest that the treatment providers in these three experiments attempted to be generally helpful to their youthful subjects. This helpfulness was indiscriminate and given as specific crisis situations arose. Less alluded to and hence less understood was the ability of treatment providers to muster attractive resources which the lower class subjects eagerly sought and used. Riding about in station wagons, tickets to sporting events, camping trips, all sorts of recreational activities--the power of such appealing activities to attract and hold the subjects was seldom discussed and never assessed. This combination of being consistently helpful and providing scarce recreational resources gave the treatment providers a certain magnetism which the subjects did not resist, but which did not alter their delinquent behavior either.

At bottom, these efforts seemed very much in accord with Dr. Cabot's naive treatment assumptions which underlay the first experiment, the Cambridge-Somerville Project. The subsequent development of more sophisticated delinquency theory obliged succeeding experimenters to cast their actual efforts in the framework of these theories, perhaps at the cost of distorting or ignoring those treatment activities which the theories could not readily accommodate or justify. As noted above, there was also the tendency on the part of the experimenters to interpret their theoretical notions so broadly as to justify practically all of the behavior of the treatment providers. Nonetheless, that aspect of treatment which may have been most appealing to the subjects, notably the aspect of recreation, received surprisingly little elaboration, quite possibly because recreation could not easily be rationalized in extant theories of treatment.

(3) Involvement

A number of experimenters attempted to assess the extent to which the subjects were meaningfully involved in the treatment programs. Their implicit assumption seemed to be that if subjects and their significant others had committed themselves to the treatment regimens, then the subjects got something useful from the treatment providers. The fact that the objectively assessed outcomes of the treatment were negative—Wincroft possibly excepted—would raise questions about the efficacy of the assumption regarding involvement. Nonetheless, it could be assumed that some subjects did better than others, and that those who began to identify with the aims of the projects probably did better than those subjects who did not get so involved. This would prove impossible to verify. Only the Cambridge-Somerville Project went to the trouble of matching each experimental subject with a control "twin," so that the progress of a particular individual could be compared with that of a non-treatment individual. In the other experiments, delinquency scores as generated by the experimental

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group were compared with the group score generated by the controls. In this situation no experimental subject was "anchored" to an external referent, and so it became unclear how any one experimental did. Simply because an experimental subject may have generated a low score during the life of the experiment did not necessarily reflect the positive impact of treatment or degree of involvement, for it could be plausibly assumed that this particular individual may have generated a low score if he had never been exposed to or become involved in treatment.

Involvement can also be seen in several other ways. As defined above, involvement meant the assessment by treatment providers of the subjects' wholehearted participation in the treatment regimen. Some projects noted other kinds of involvement. At least one project observed that some subjects intensively utilized the resources made available through the experiment, and in this sense were intensively "involved," but shunned, in the treatment providers' estimations, the actual aim of the experiment: the modification of manifest delinquent behavior. And involvement could mean simply finding treatment regimens that were sufficiently attractive to keep the subjects' interest. Here, involvement meant nothing more than having activities which at least kept the subjects exposed to the treatment providers. This latter sense of involvement seems to be what was most often described, although this meaning generally got inextricably mingled with involvement defined as a subject's degree of commitment to programmatic aims.

In three projects, Cambridge-Somerville, Wincroft, and Youth Consultation, the treatment providers were asked at some point to rate the involvement of the subjects in the treatment programs. In 1940, the Cambridge-Somerville treatment providers rated only 22 of 322 subjects (7 percent) as overtly resistant to treatment; an additional 48 subjects (15 percent) were seen as needing little service and so were uninvolved through choice of the treatment provider. Wincroft treatment providers said that 14 out of the 54 experimental subjects (26 percent) were minimally involved, but which of these were uninvolved through personal or provider choice is not made clear. Youth Consultation treatment providers found that subject involvement varied with treatment regimen. Approximately half of the first cohort of 47 subjects who received individualized casework were seen as uninvolved. The switch to group services produced better results; of 127 subjects rated, 51 subjects (40 percent) were very involved in group treatment, 51 subjects (40 percent) were involved some or little, while 25 subjects (20 percent) were hardly or not at all involved. In these three experiments between 20 percent and 60 percent of the subjects were minimally involved

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in the sense of being enthusiastically attracted to the treatment regimens.

Youth Consultation's experience with individualized casework services appears duplicated in the Maximum benefit Study, which assessed the majority of families selected for treatment as "uncooperative," and the New York City Youth Board Study, which found the parents of the most disturbed subjects to be unreachable. It appears that where individual treatment or casework having an ego-psychological orientation was employed, subjects and their significant others were resistant. This underscores the frequently noted impasse created when treatment providers who believe they have a viable remedy meet subjects who refuse to submit to those providers. Cambridge-Somerville appears as the one project which individualized subjects without courting their alienation, but in this instance, individualized treatment was not necessarily in the ego-psychological mode.

Group-oriented treatment programs were better received and involvement, in the simple sense of being attractive to subjects, was much higher. Midcity and Chicago Youth Development accepted established adolescent peer groups as given, and attempted to redirect the activities of friendship groups. Seattle Atlantic Street Center, Wincroft, Youth Consultation Service, and the Youth Development Projects recognized the attraction of peer groups, but attempted to structure groups deliberately in order to maximize treatment impact. Particularly where recreation was a part of the treatment regimen and made available to groups, involvement appeared to be high. If subjects resisted casework, they may have ignored or redirected treatment aims when groups and recreations were utilized. The Chicago Youth Development Project noted that often the highest users of group and recreational resources were subjects who had no apparent commitment to personal change. This is a variation of the impasse noted earlier; these subjects were willing to be in high proximity to treatment providers because of the resources available but were indifferent, and perhaps scornful, of treatment aims the providers tried to hold forth.

(4) The Wincroft Youth Project

With the Wincroft Project the sole delinquency prevention experiment claiming some significant success, it must be asked how in a treatment sense this project differed from the others. Wincroft employed an altogether unique treatment stratagem which, unfortunately, was little discussed in the final report and so is not altogether understood. While the project designated 54 experimental subjects, the experimenters saw the adolescent youths in the Wincroft area as all worthy of attention, some because they were the peers of the experimentals and others because they one way or another may have had influence, however remote, upon the experimentals. The treatment that evolved amounted to nothing less than attempting to manage a significant amount of the spare time activities of youths in a lower class neighborhood. Wincroft is the only project to use volunteers to assist in treatment, and over the life of the project organized and trained better than 150 adult volunteers who brought service to approximately 600 youths. Direct services to the experimentals amounted to only about 30 percent of the total treatment effort. On any particular night in Wincroft, at least several project-sponsored youth groups were meeting. This was a comprehensive community treatment effort which no other experiment matched.

Also notable in the English experiment was the lack of concern about school achievement and employment prospects. While school performance and attendance obsessed American treatment providers, their English counterparts were less invested in these aims because working and lower class English youths were neither required nor often expected to attend school beyond the fifteenth year. Furthermore, when school ended there appeared available a source of low-paying, low-skilled jobs which the Wincroft youths moved into and out of some facility. Employment for young American school dropouts, by contrast, was generally not available and so posed a problem treatment providers could not satisfactorily address, and as in the Chicago Youth Development Project amounted to a bitter failure. With school failure and lack of employment less crucial in the English scene, the English treatment providers were free to concentrate on what they could probably do best--counsel, reconcile, facilitate, and unabashedly provide recreation.

Finally, the English subjects were unlike the treatment providers, including the volunteers, in social class affiliation--the subjects were lower class, the providers all middle class--but in most other respects subjects and providers were culturally similar. With so many of the American subjects being lower class blacks and Puerto Ricans and the treatment providers being apparently white and middle class, the cultural and societal gulf appeared more difficult to bridge. The Chicago Youth Development Project found the adjacent white community openly hostile to the project's work with black subjects. When in addition to delinquency, American treatment providers had to address a range of educational, employment, racial, and cultural problems, their task was formidable indeed.

G. Findings

No further elaboration of the findings will be made here. Having been much alluded to throughout and with the research procedures assessed in a previous section, the findings need no further discussion.

H. Recommendations

At their close, some experimenters, reflecting upon their lack of success, advanced recommendations for future courses of action. Seen together, these recommendations do not converge toward a particular point of view, so no consensus emerges from the various unsuccessful efforts. Some recommendations were quite modest; the Youth Development Project, for example, recommended that more sensitive ways be developed to measure possible treatment impact so that an almost total reliance upon such "crude" measures as school and police files could be augmented. Some recommendations appear contradictory. The Youth Consultation Project found that attempting to work with youths before a clear antisocial behavioral configuration developed made treatment vague and unfocused, the implication being that older children with tangible problems might make better subjects. On the other hand, one of the Chicago Youth Development Project's recommendations was to work with younger children because their experience working with older adolescents who had been through the juvenile justice systems showed such adolescents to be hardened and resistant to change. Only the New York City Youth Board Glueck Validation Project recommended reaching down to pre-kindergarten children in order to help in the socialization of vulnerable children and to prevent delinquency.

A recommendation shared by Cambridge-Somerville and the Youth Consultation Service was one which said the prevention of delinquency should not be the primary aim of treatment given lower class children. For these children concrete services and counseling were justified in their own right. The Maximum Benefits Project, however, would give legal power to the preventers of delinquency; it was recommended that unsocialized families be mandatorily made to live in a "therapeutic sub-community" or "family hospital" where normative child-rearing skills had to be learned and practiced.

Three projects called for environmental change if delinquency was to be prevented. Atlantic Street Center, Youth Consultation Service, and Chicago Youth Development all noted the sheer weight of negative factors which impinge upon ghetto and lower class life, and which make the individual efforts of well-meaning treatment providers seem insignificant. More legitimate opportunities for alienated youth to find employment, family income guarantees, and the like appeared to be what these experimenters considered necessary before individual and group treatment could be successful in preventing delinquency.

I. Conclusion

The ten experiments reviewed here probably represent our best efforts to date to prevent delinquency. The dedication of the project personnel in each of these experiments was evident; the very nature of the experimental design would have prompted the best efforts of the treatment providers.

The rigor and honesty with which each experiment was evaluated, and the convergence of the evidence in a negative direction leaves little room to doubt that as yet we do not know how to prevent delinquency. The hope is that this review and analysis may serve others who wish to build upon what is known as they continue to address the problem.

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